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CONTENTS

MAIN ARTICLES

The Development of the Concept of Cerebral Localization in the Nineteenth Century: A. EARL WALKER 99

James Mease, M. D., on the Diseases of Domestic Animals: J. F. SMITHCORS 122

AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE: Twenty-Ninth Annual Meeting (Cont'd.):

John Mitchell, Benjamin Rush, and Yellow Fever: SAUL JARCHO 132

Benjamin Franklin and the Rise of Free Treatment of the Poor by the Medical Profession of Philadelphia: ROBERT J. HUNTER 137

Deborah Sampson, *alias* Robert Shirtliff, Fighting Female of the Continental Line: WM. FREDERICK NORWOOD 147

The Famous Harrison Case and Its Repercussions: LINDEN F. EDWARDS 162

Charles E. Morgan's *Electro-Physiology and Therapeutics*: an Unknown English Version of du Bois-Reymond's *Thierische Elektricität*: PAUL F. CRANEFIELD 172

NOTES AND COMMENTS

Nature and the Libido: a Case of Semantic Continuity: WILLIAM D. SHARPE 182

MEDICO-HISTORICAL NEWS AND ACTIVITIES

Announcements 186

BOOK REVIEWS 188

CONTRIBUTORS TO THIS NUMBER 196

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THE DEVELOPMENT OF THE CONCEPT OF CEREBRAL LOCALIZATION IN THE NINETEENTH CENTURY *

A. EARL WALKER **

Introduction

For centuries the learned disciplines discussed the mechanisms and seat of the psychic phenomena which comprise the mind. Many theories were evolved, the majority, both in classical and modern times, regarding the nervous system more or less as the substrate of the intellectual faculties. By the eighteenth century there was a general acceptance of the thesis that the sense organs subserving vision, audition, olfaction, gustation, and touch but not pain, which was considered an emotion, were connected, or at least related, to a common pool in the brain—the sensorium commune—which was variously located in the cerebral ventricles, brain stem, corpus callosum, or white matter of the brain. Haller (35), who considered the cerebral white matter as the seat of sensation from which the will initiated movements, did not believe the psychic functions could be

* The Merck Lecture, presented at the University of Saskatchewan, Saskatoon, Sask., November 5, 1956. It is a privilege to acknowledge the appreciation of physicians and surgeons in general to the pharmaceutical profession which has so advanced the practice of medicine, and to thank personally one of its worthy representatives for which this lecture is named and the medical faculty of the University of Saskatchewan for the honor and privilege of appearing before this University body.

** From the Division of Neurological Surgery, Johns Hopkins University School of Medicine, Baltimore, Md.

assigned to particular parts of the brain. Bichat (6), the brilliant French pathologist, admitted the brain as the sensorium commune but held that the organs—liver, spleen, etc. played a role in the production of the passions or emotions, thus fear arose from the stomach, anger from the liver, etc. Soemmering (70), impressed by his anatomical findings that the cranial nerves all terminated in the walls of the ventricles where they were bathed in ventricular fluid, concluded that this liquor was the medium of nervous activity, the sensorium commune and the seat of the soul. The philosopher Kant (44), to whom Soemmering's treatise was dedicated, admitted the possibility of a sensorium commune although an aqueous base for this seemed unlikely since it did not have the potentialities for organization which such a center should theoretically possess. Moreover, Kant denied that the soul could be spatially localized and thought that in seeking its seat, one was attempting the impossible.

An appreciation of this metaphysical concept of mind prevailing at the opening of the nineteenth century is essential to an understanding of the developments in the past 150 years. It is paradoxical that the stimulus for the concept of cerebral localization should have been derived from a pseudoscience which came to be ridiculed by the scientific world. But phrenology, as propounded by Gall (30) and his pupil Spurzheim, did have in its unorthodox, exaggerated and fanatical propositions certain elements of truth. For this reason, the general principles of Gall and Spurzheim deserve consideration.

As a boy, Gall had noted that some students having a remarkable memory for words had prominent eyes. Did not the faculty for verbal memory then lie above and behind the orbital cavity? And if that was so, would not other mental faculties be associated with bumps or prominences on the surface of the skull? Such was the premise for his investigations but in his pursuit of this problem he lost all critical sense.

Gall, however, did emphasize that it was the gray matter, the cerebral cortex, in which were the organs of the mind—not one but a concatenation of organs which were connected by commissures in the white matter. The organs or faculties were conceived as independent and each represented topographically in the cerebral cortex. It was this dissection and topical organization of the psyche which drew the scorn of contemporary writers, one of whom penned in satirical vein: "Without any line of demarcation in the brain to answer to the trellis-work of the craniological specimen, we enter the most alien-domains. . . . From *pugnacity* we enter *friendship*, without a turnpike between. . . . *Building* and *music*,

though their noises are so distinguishable, almost occupy a common ground. . . . We may, perhaps, enquire into the proof that these organs are so strangely figured, as well as uncouthly collocated. Here are angles acute and obtuse; triangles, right-angled, isoceles, and scalene; straight

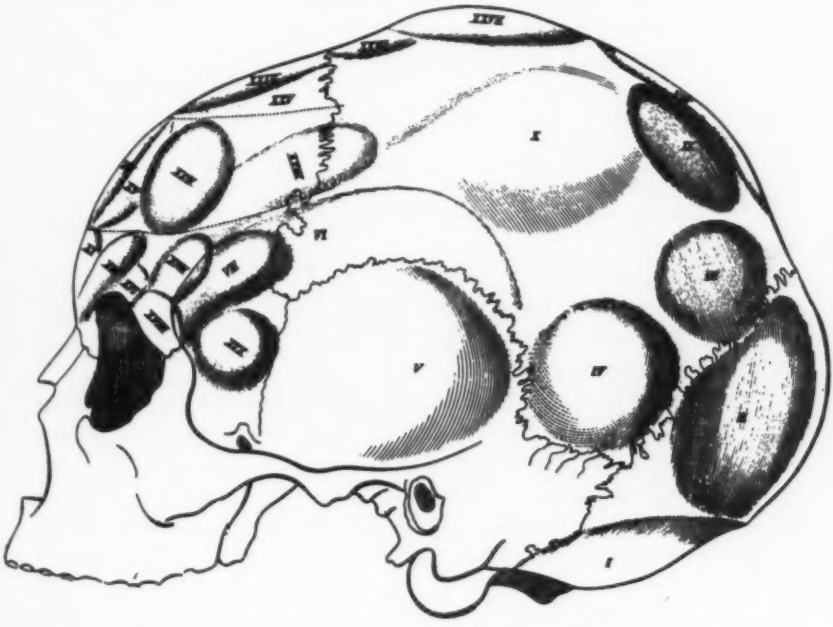

FIG. 1. Gall and Spurzheim's cranial map of human moral and intellectual qualities.

I. Sexual	X. Caution	XX. Wisdom
II. Love	XI. Memory of things	XXII. Caustic retort
III. Friendship	XII. Sense of space	XXIII. Poetic
IV. Self defence	XV. Language	XXIV. Morality
V. Meat lover	XVI. Painting — colors	XXV. Mimicry
VI. Cunning	XVII. Music	XXVI. Religion
VII. Ownership	XVIII. Mathematics	XXVII. Constancy
VIII. Pride	XIX. Mechanical-Architectural	
IX. Vanity		

F. J. Gall and G. Spurzheim, *Anatomie et physiologie du système nerveaux en général, et du cerveau en particulier*, Atlas, Paris, 1810, plate XCIX.

and curved lines; cones and circles; rhomboids, trapeziums, and polygons. Now as there is nothing *very* like this upon the brain, or the skull, it may not be impertinent to ask how they have been discovered? He must have had ingenuity at least who drew these amorphous etchings of the head" (36, p. 73).

Although Gall and Spurzheim gained a popular following, their ideas on craniology were so absurd that some critical scientists regarded the entire thesis with contempt. Hence, Flourens' researches, which seemed to disprove Gall and Spurzheim's doctrines, were uncritically accepted by the academic world. Flourens (25) described how he exposed a dog's hind brain, and by pricking or pinching the brainstem successively more rostrally found a point at which muscular contractions no longer followed the mechanical stimulation. In another dog he irritated the anterior parts of the brain extending backwards until twitchings occurred. This point—in the region of the quadrigeminal plate—coincided with the level at which muscular contractions had ceased. Flourens naturally assumed that the lower brainstem had a motor function and that the cerebral hemispheres were inexcitable.

This conclusion seemed to be confirmed by Flourens' observations after ablation of parts of the brain, for he found as he removed the hemispheres that more and more of the various intellectual faculties were lost until, with decortication, they were destroyed.

After hemispherectomy, he stated, the pigeon was in a perpetual sleep. The bird remained motionless wherever placed, and only opened its eyes if disturbed and then lapsed back into slumber. It could swallow if fed, but would starve in front of corn. If thrown into the air it could fly but would bump into obstacles. Thus Flourens thought the bird, though capable of motion by brainstem centers, was devoid of its perceptions, instincts, and intellectual faculties. Later investigators were to show that Flourens' surgery had probably been too radical and if the excisions were confined to the hemispheres, the birds and other animals did not have a complete loss of sensation but could see, hear, and feel. However Flourens' authority in the scientific world was so great that his views on the diffuse unlocalizable functions of the cerebral hemispheres were held for half a century.

But the phrenological thesis, which Flourens decried, was destroyed only in part. Craniology was buried, but the doctrine of cerebral localization was taking root in both experimental and clinical laboratories.

Although Flourens in his extensive studies of stimulation and destruction of the cerebral hemispheres concluded that the gray substance was a homogeneous equipotential mass in which thought, memory, volition, and perception had a common anatomical basis, not all physiologists concurred in its homogeneity of function or in its representation of the intellectual faculties commonly regarded as mind.

To quote from Bostock's physiology of 1828: "Do the phenomena of mind bear such a resemblance to those which are usually ascribed to matter, as to justify us in placing them in the same class, and, secondly, is the relation between the condition of the brain and the state of the intellect, such as to indicate that a necessary connexion exists between them?" (7, p. 153). And the conclusion, "I conceive myself warranted in drawing the inference, that mind is not a property of the brain" (7, p. 157).

And scattered observations had and were being made, that the cerebral cortex which Flourens had considered inexcitable might upon adequate irritation give rise to movements of the body or extremities. Zinn (76) in one experiment noted convulsions when the anterior part of the hemisphere of a dog was incised, although in most of his experiments the cerebrum was inexcitable and insensitive. Then Cabanis wrote "Si l'on pique ou si l'on irrite d'une manière quelconque différents points de l'organe cérébral, on voit les convulsions qui sont ordinairement produites par ce moyen passer tour à tour d'un muscle à l'autre, et souvent ne pas s'étendre au-delà de ceux qui se rapportent aux points irrités" (14, p. 186).

Again Eckhard (21) stated that Renzi (61) had noted movements in the forelimbs when cutting the white substance of the forebrain. But Flourens (25) and a multitude of other investigators (50, 53) at the beginning of the 19th century failed to obtain such responses.

The first clear description of muscular response due to electrical stimulation of the cerebrum comes from Rolando (62) who states that he observed contractions when one conductor from a voltaic pile was introduced into the cerebral hemisphere of a pig. True, he noted that the most violent muscular responses arose from stimulation of the cerebellum. Moreover, Rolando concluded that the cerebral hemispheres contain a group of fibers producing voluntary movement. However, the voice of authority from the French Academy belittled this observation, for Flourens (25) wrote: "Rolando appears to have induced such responses—but they were due to the conduction of the galvanic current directly 'aux parties excitatrices immédiates de la contraction.' My experiments establish that the hemispheres of the brain do not produce any movement" (25, p. 274).

Clinical evidence was accumulating to indicate that the cerebral cortex was not equipotential. In 1825 Bouillaud (8) published a paper entitled "Clinical researches to demonstrate that loss of speech corresponds with lesions of the anterior lobules of the brain, and to confirm Gall's opinion on the seat of the organ of articulate language." In this report he de-

scribed a number of cases of aphasic speech disturbance and showed that on postmortem the lesions involved the orbito-frontal lobe. So sure was he of his conclusion, that he offered to pay 500 francs to anyone who could satisfactorily demonstrate that speech disorders were not associated with lesions of the frontal lobe.

M. Dax (17) noted that disorders of speech were associated with left hemisphere lesions—a conclusion which was not readily or unanimously accepted. However, Bouillaud pointed out that a number of highly coordinated acts were ordinarily associated with the right hand. This, he stated, implied special functions for the left hemisphere and he further argued that speech, too, should be located there.

In 1861 Broca (9) presented to the Anthropological Society of Paris a memoir in which he stated, on the basis of his clinical experience, that lesions of the left inferior third frontal convolution produced a loss of the faculty of speech but not a paralysis of the muscles employed in phonation.

It was this sagacious reasoning of Broca that laid the foundation stone of the new theory of cerebral localization. Fortunately, the groundwork was well settled, for Broca's cornerstone could not have weathered a storm of critical analysis. The crucial brain upon which he based the proof for his theory of "aphemia," proved to have in addition to a lesion of the third frontal convolution, which Broca had prophesied and gleefully demonstrated, more extensive cerebral involvement which only came to light years later. But these complicating lesions were revealed long after the general concepts of cerebral localization were well established. Broca was aware of the importance of his pronouncement to the theory of cerebral localization for he said: "We now know that all parts of the brain properly so-called have not the same functions, that all the convolutions represent, not a single organ, but many organs or groups of organs and that there are large distinct regions of the brain which correspond to the regions of the mind." Thus, the theoretical framework was ready for the mapping out of motor, sensory, auditory, visual, and other cortical areas.

It is of interest to note that the phase of neurology which has contributed so greatly to the knowledge of the representation of function in the cerebral cortex in recent years—namely, epilepsy—was not exploited in the beginning of the nineteenth century. True, Bravais had clearly described the march of the focal epileptic seizure but knew not of its cerebral origin. Even Hughlings Jackson, whose keen analytic brain disclosed so much of the workings of the nervous system, thought that

unilateral convulsions came from the basal ganglia, until in 1870 a new technique—repetitive electrical stimulation—replaced the other methods of irritation, pinching, pricking, cutting, and continuous galvanic shocking of the nervous tissue, and settled the question of the excitability of the cerebral cortex.

Through the thunderbolts of the Gods, man first became aware of electrical stimulation. That this divine lightning could be home-made was only realized a little more than two hundred years ago, when scientists in Leyden felt the shock of a charged glass of water. This was the famed Leyden jar, the jolt from which could lay up a man for two days (60). It was natural, of course, that this new and stimulating discovery should be hailed as a therapeutic agent and viewed by enthusiasts and charlatans alike as a remedy for all types of disease. Even Benjamin Franklin (28) used it in a number of paralysed patients, noting that it "gave great spirits to the patients and made them hope of perfect cure," although he admits that "in palsied I never know of any advantage from electricity that was permanent and how far the temporary advantages might arise from exercise or from the spirits given by the hope of success enabling them to exert more stress in moving their limbs, I shall not pretend to say."

In those early days of electrical research, biological material was used to detect the presence of an electric charge (31). The excised muscle of a frog or the tongue of a man in the 18th century was a much better detector of electric current than any other instrument. In fact, the freshly killed frog's leg responded to a current 50-60 times weaker than that which could be detected by the most sensitive electrometer of that day; hence for some years the nerve muscle preparation dubbed by Volta (74) "the animal electrometer" occupied an important position in the physicist's laboratory. And more than a century later, neurophysiologists were tasting their stimulating electrodes to determine the threshold for nervous excitation (29).

Modern Stimulation Studies

It is interesting to observe how some of the most fundamental and important discoveries throughout the ages have been made under the most humble circumstances. Consider Anton van Leeuwenhoek peering into a drop of rain water through his home-made microscope, Isaac Newton wondering about the falling apple, James Watt observing the teakettle sign, Benjamin Franklin flying his kite in the storm, and Galvani's twitching frog's muscles exuding "animal electricity." So it is not surprising

that the birth of modern cortical stimulation and ablation occurred on a dressing table in a small Berlin home, because the University had no space for such studies (40). And the investigators were, of course, young men under thirty, for more experienced scientists would have been awed into inactivity by the austerity and authority of the scientists who had maintained for over a half-century that the cerebral cortex was inexcitable. One of these young fellows, a venturesome chap, after presenting his doctorate thesis on " De medullae spinalis textura " had gone to South Africa for three years to study the anthropology, sociology, and ethnology of the natives. Moreover it is said that he had observed, while dressing a head wound some years earlier, that mechanical irritation of the brain caused twitching of the contralateral limbs. His companion, Eduard Hitzig, had seen, upon stimulating with galvanic current the temporal and occipital parts of the head, movements of the eyes which Hitzig reasoned might be due to spread to the quadrigeminal plate, or to the cerebral cortex. The former assumption was in line with current physiological thinking but not the latter, for the motor system was considered to stem from the basal ganglia. Todd * (71) in 1855 put it thus: " I would say that the centre of volition is of very great extent: it reaches from the corpora striata in the brain down the entire length of the anterior horns of the gray matter of the spinal cord, and includes the locus niger in the crus cerebri, and much of the vesicular matter of the mesocephale and of the medulla oblongata " (71, p. 19).

But this did not daunt Fritsch and Hitzig (29) who after a preliminary positive test on a rabbit made a series of experiments on dogs. From the information given, one can only guess at the arrangement of the stimulator used; it seems probable that a current about 1 milliampere, interrupted manually, was passed through the cortex with a maximum of 11 volts. Using bipolar platinum electrodes stuck through a cork the entire convexity of the dog's cortex was explored; the anterior region gave rise to motor responses but the posterior parts were inexcitable. Stimulation of the frontal " motor " area gave rise to muscular contractions of the limbs opposite to the side stimulated. With weak currents, the responses were quite discrete, but with stronger stimuli the muscular

* This was the illustrious English neurologist, R. Bentley Todd, whose name is linked to the post-epileptic focal paralyses. Six years earlier he had observed motor responses from stimulation of the cerebral hemispheres by a " magneto-electric rotation machine." However, Todd, too intent upon locating the seat of epileptic phenomena, did not recognize their significance and hence missed an opportunity to discover the function of the cerebral cortex.

response was more widespread and even of muscles ipsilateral to the stimulation. Using feeble stimuli it was possible to elicit specific movements of an extremity—flexion or extension of a paw. If the current was increased excessively a generalized convulsion developed.

To substantiate that such motor centers existed, Fritsch and Hitzig proceeded to demonstrate that removal of these centers would disturb

FIG. 2. The motor centers of the dog as mapped by Fritsch and Hitzig. M. Foster, *A Text Book of Physiology*, 2nd ed., London, Macmillan, 1878, p. 501.

Triangle—neck movements
Cross—forelimb movements
Square—hindlimb movements
Circle—face movements

motor power of the opposite limbs. Accordingly, after defining the motor center of the right forelimb by electrical stimulation, they removed it in two dogs. Upon recovery from the anesthetic the animal in walking raised the right forelimb inappropriately and slipped on that side as if to fall. At times it stood upon the dorsum of the right forelimb and when sitting with the forepaws resting in front, it would tend to fall to the right.

Thus, in a Berlin home, the doctrine of localization of cerebral function was established by experimental data based upon both stimulation and ablation.

Although shortly afterwards confirmed by Ferrier (23), it was not accepted universally. Bitterly, Burden-Sanderson (65) assailed the conclusions, demonstrating to his satisfaction that the results were due to spread of current to the striatum then considered a motor ganglion. Carville and Duret (15), working in Vulpian's laboratory, similarly maintained that the basal ganglia were the source of the motor responses.

Even the confirmation and elaboration of these findings did not settle entirely the issue. For Michael Foster (27) in 1878 writes:

> These experiments leave no doubt that there is a connection between galvanic stimulation of the brain surface and bodily movement; but the exact nature of this connection is at present obscure. . . .
>
> It seems a priori unlikely that such valuable material as we must needs suppose the grey matter of the convolutions to be, should be taken up in such, so to speak, menial work as bending or straightening a limb; and the results of subsequent observers have largely diminished the value which was at first attached to Hitzig's observations. Not only do the phenomena continue when the animal is under opium and chloroform, provided that the anesthesia is not too profound, and not only do they require for their development currents of a considerable strength, mechanical and chemical stimulation being unable to produce them, but the results of stimulation are the same, when the surface of the convolution operated on is highly congested, and even when it has become completely dried up, or after it has been washed with strong nitric acid. The results, moreover, remain unchanged when the area experimented upon is isolated from the surrounding grey matter, by plunging a cork-borer for some distance into the brain round it; and even when the brain substance is removed to some depth down by means of the cork-borer, and the electrodes plunged into the blood which fills up the vacant cylindrical hole. They remain the same when the surface stimulated is disconnected physiologically though not physically from the deeper parts by a horizontal incision carried some little distance from the surface. And though the area, stimulation of which gives rise to a definite movement, is always limited, yet it is not constant in different individuals, and frequently a large and deep sulcus may be seen running through its very midst. All these facts suggest that the results are due to the escape of the current from the surface to which the electrodes are applied to deeper underlying portions of the brain, the escape taking place along definite lines determined by the electrical conductivity of the brain substance. And Burdon Sanderson states that local stimulation of the white matter immediately surrounding a corpus striatum produces localized movements quite similar to those caused by stimulation of the corresponding cerebral surface; from which it may be inferred that when the surface appears to be stimulated, it is really the corpus striatum which is affected physiologically by the stimulus" (27, p. 505).

However, it was not long before the general principles of Fritsch's and Hitzig's research were firmly established. The further studies of Hitzig (39) in Germany, of Ferrier (24) in England, Luciani and Tamburini

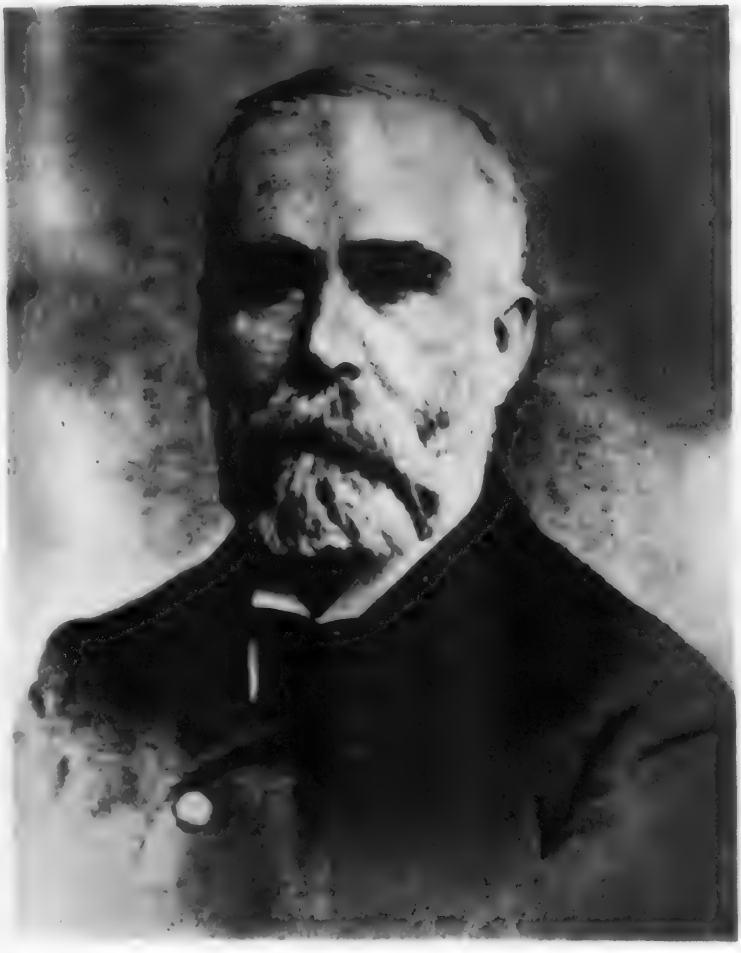

FIG. 3. DR. ROBERTS BARTHOLOW

(Courtesy of
National Library of Medicine,
Washington 25, D. C.)

(52) in Italy, and others fully confirmed the existence of a cerebral motor center in dog and monkey. And the confirmation of its presence in man awaited only a fortuitous occasion and a courageous physician.

Early in 1874 these conditions were met in Cincinnati, Ohio. A feeble-minded servant girl was dying of a malignant purulent ulcer of the scalp. Her attending physician, Roberts Bartholow (1), Professor of Medicine and a prominent Cincinnati practitioner, obtained her consent to insert fine insulated wires in order to stimulate the cerebral matter which was covered only with granulating dura mater. In the first observation the sensitivity of the dura mater and the insensibility of the underlying brain when touched were demonstrated. When the needles were inserted approximately just into the dura mater of the left postcentral convolution (as later verified) and the faradic current applied, muscular contractions occurred in the right arm and leg. The arm was thrown out, the fingers extended, and the leg projected forward. The head was turned to the left. Similar effects on the opposite side were seen when the right parietal region was stimulated.

When one needle was inserted further into the left brain and the second just through the dura mater, stimulation produced similar muscular contractions and strong unpleasant tingling in the right limbs, especially the arm. With the needle similarly placed in the right parietal lobe and the strength of current increased, the patient exhibited great distress and after the above contractions developed a generalized seizure which lasted for five minutes.

Later faradic stimulation of the parietal lobes was repeated with the same results. Two days later, however, the patient had a right hemiparesis and passed into coma. The following day she had a convulsion and died. At autopsy a thick layer of greenish-yellow pus extended over the whole of the left hemisphere, and the sagittal sinus was thrombosed.

Bartholow presented these facts, as he stated, without comment but this experimentation upon the human brain was not passed without comment by others. Both in this country and Europe criticism was levelled at this audacious experimentation. The editor of the *British Medical Journal* (22) asked: "Is nothing to be said from another point of view which is not physiological?" To which Bartholow (2) wrote that he had carried out the studies with the patient's full knowledge and consent and under the impression that the technique would not be harmful. But he added: "To repeat such experiments with the knowledge we now have that injury will be done by them—although they did not cause the fatal result in my own case—would be in the highest degree criminal.

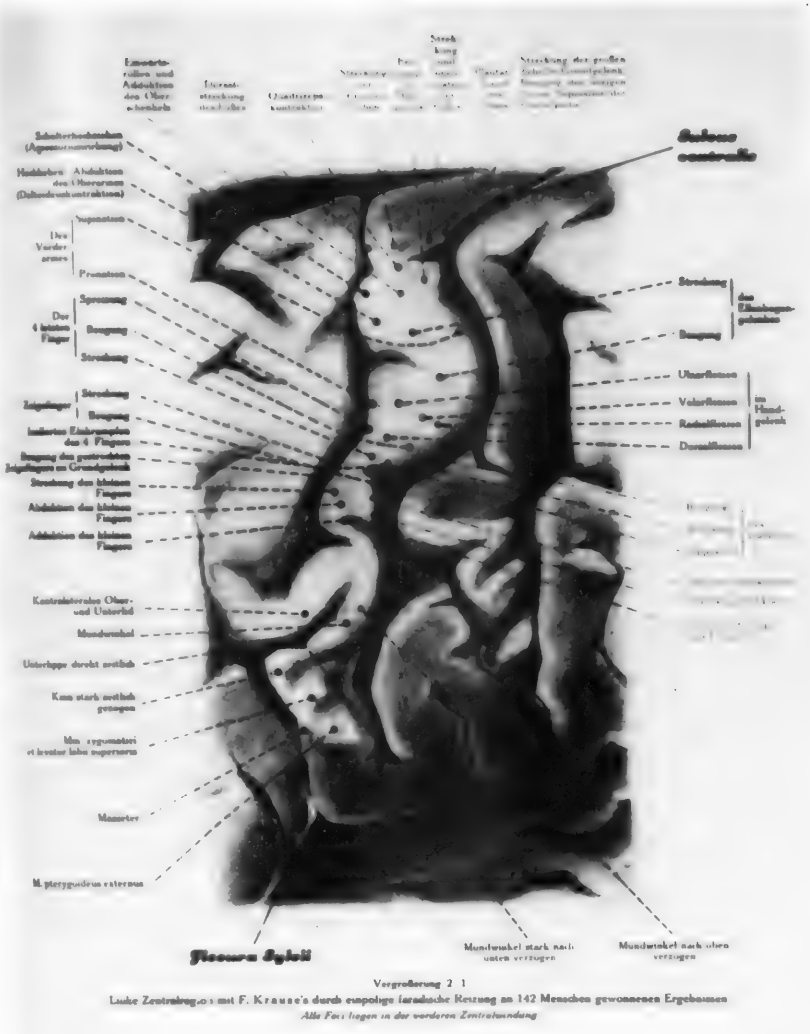

Fig. 4. Krause's map of the motor cortex based upon monopolar stimulation of 142 patients. F. Krause and H. Schum, *Die spezielle Chirurgie der Gehirnkrankheiten*, II. Bd., Stuttgart, Enke, 1931, facing p. 454.

Yet eight years later Sciamanni (68), under similar circumstances and with the same clinical result, stimulated the inferior central region inducing facial movements.

In the meantime, laboratories were buzzing as the inductorium stimulated the cerebral cortex of many animals under a variety of conditions. Ferrier (24) carefully mapped the cortex of the monkey (Macacus cynomolgus) finding that the excitable area extended over the precentral convolutions, the parietal lobe and the superior temporal gyrus. Horsley (42), too, explored with the electric current every brain he could get his hands on by hook or crook.*

With Schäfer, Horsley (42) mapped out the excitable area of the cortex of monkey and orang-outang, finding both pre- and postcentral convolutions excitable. In contrast to this observation were those of Grünbaum and Sherrington (34) who stimulated by unipolar faradization some 16 anthropoids—orang-outangs, gorillas, and chimpanzees, reporting that with the exception of the eye fields the excitable areas were confined to the precentral convolutions. Finally, this era should not be left without mention of Krause's (48) excitation of the human cerebral cortex and his demonstration of the topographical orientation with the precentral gyrus.

But this discussion would not be complete without mentioning the negative effects of stimulation. In 1881 Bubnoff and Heidenhain (13) reported that the powerful contraction of the extensor muscles of the dog's toes could be relaxed by a stimulation of the motor cortex with a weak current. About the same time, in 1884, Brown-Séquard (12) noted that excitation of the non-motor cerebral cortex of dogs and rabbits might abolish the excitability of the motor areas for minutes. These inhibitory reactions were further studied and their mechanisms elaborated by Sherrington (69), whose demonstration of reciprocal innervation of antagonistic muscles is a classic.

* It is said that Horsley, who in his intense preoccupation with his scientific endeavors sometimes allowed his enthusiasm to override his judgment, one day burst into the lecture room of a colleague, who had recently returned from Egypt with a greatly prized camel.

"Sh – – sh" whispered Horsley, "Your camel- "

"Well, what about my camel?" replied the professor, annoyed at the interruption to his lecture.

"It's dead," said Horsley.

"What?" bellowed the professor, well aware of Horsley's weakness and fearing the worst—"You don't mean to say you operated upon it?"

"Who could do it better than I?" was Horsley's indignant retort.

It may seem strange to modern students of neurophysiology that, to this point, no mention has been made of the structural basis for the cerebral centers. Indeed, the cytoarchitectural studies of the cerebral mantle were just beginning, and neurophysiologists were much too excited about their thrilling motor responses to pay much attention to the descriptions of Betz (4), Bevan Lewis (49), and others. A new generation of investigators early in the twentieth century brought together these anatomical and physiological findings.

It was the Vogts (72) who first attempted a detailed correlation between cortical stimulation and the cytoarchitectural appearance of the cerebral cortex. As they stimulated the monkey's cortex they noted, sometimes by a cut, the place at which a change in response occurred so that they could determine the cytoarchitecture when the brain was later sectioned. In this way their architectonic mapping was, if not dependent, at least greatly influenced by their stimulations.

From the Vogts' studies of the correlation of structure and function of the cerebral cortex stemmed much of the subsequent research. Certainly Foerster's (26) investigations of the human cerebral cortex were stimulated by and his findings perhaps biased by the Vogt's reports. The prototype of his cortical map would seem to have been the Vogt's concept of the human brain based upon analogies made from the monkey cortex. Foerster's observations stimulated Wilder Penfield, John Fulton, and Paul Bucy to analyse the organization of the cerebral cortex of many primates including man.

Finally, some reference should be made to depth stimulation. For some time it has been known that wires might be embedded within the brain to excite the subcortical structures. In Zurich, Hess (38), about 1930, developed a technique for inserting electrodes into the depth of the brainstem and bringing the connecting wires through the skin. In order to excite without wires being in the way of the movements of the animals, Chaffee and Light (16) devised a technique for remote stimulation using the principle of electro-magnetic induction between an external primary and a buried internal secondary coil. At almost the same time Loucks (51) used a similar technique, implanting small coils beneath the cervical skin of dogs in order to study conditioned reflexes.

Modern Ablation Studies

While the reactions of the brain to electrical stimulation were being studied in animals, the localization of function within the cerebral cortex

was being pursued further by another technique, namely ablation. For the location of the cortical sensory representation, the excision of brain tissue proved somewhat more satisfactory than excitation. However, the crude surgical techniques of the early investigators and the lack of any histological control produced discrepancies in their findings. Reliable studies of the effect of cerebral ablations were not made until the end of the nineteenth century when Goltz (32) carried out a series of investigations which clarified the issues. He demonstrated that the decerebrate frog can hop, swim, jump out of boiling water, croak like any frog, and is able to adjust itself mechanically to external stimulation, but will otherwise sit and starve. If the optic thalami remain, the animal may attempt to feed itself and exhibit a sexual instinct. Goltz's descriptions, more vivid and precise than those of previous workers, emphasized that the effects of decerebration are more profound the higher the animal stands in the phylogenetic scale.

Perhaps the most important of all the observations upon decortication and certainly the best known, were those made by Goltz (33) on a brainless dog, which was exhibited at various medical meetings throughout Europe. He succeeded in decorticating two dogs and keeping them alive for periods of 57 days and 92 days. A third and famous dog lived 18 months with no structures above the tentorium other than a markedly shrunken left anterior quadrigeminate body, a small soft greyish mass 1.7 cm. in length representing the left corpus striatum and thalamus, the atrophic right thalamus and corpus striatum, and a small amount of the cornu Ammonis. For a few months after its last cerebral ablation the dog had a marked disturbance of nutrition. This was attributed by Goltz to the animal's restlessness, its intervals of rest and sleep being less than a normal dog's, and partially to imperfect thermo-regulation. During the entire period of observation the animal showed no sexual desire. When walking, it easily slipped on a smooth floor, although it was capable of recovering itself. It never walked on the dorsa of the feet. If its limbs were placed in an abnormal position this was corrected. After hurting one of its hind limbs it trotted about on three sound limbs, holding up the injured member. If irritated at some point in its extremities, the animal would turn its head sharply in that direction and snap but seldom reach the offending object. The animal apparently had some blunting of the sense of touch. The sense of taste remained, at least to very bitter objects. The sense of hearing appeared to be much reduced, although the blare of a trumpet would arouse the animal from sleep. Vision was somewhat im-

paired, the dog being unable to avoid objects by sight, although it did respond to a flash of light and would turn away from a very bright light. The pupils contracted sharply to light. The animal remained mute and indifferent to caresses and threats. It did not, however, lose its sense of hunger or instinct to feed. If a dish of milk were placed near its nose it would lap, chew, and swallow with satisfaction. However, the animal did not exhibit any desire to seek food. Rothmann (64) confirmed the work of Goltz, keeping a dog alive for over two years. Karpus and Kreidl (45) extirpated the cerebral hemispheres of two monkeys which survived for two weeks after the extirpation of the second hemisphere.

Occipital lobectomy. When it became apparent that the cerebral cortex was not a homogeneous structure but contained areas having more or less specific functions, regional excisions of the cortex were studied once more. The visual impairment in Flourens' pigeons was a prominent feature of their disability but it was not until 1855 that De Panizza (58) noted this defect following posterior cerebral excisions and traced the pathway from the optic nerve to the occipital cerebral cortex. Hitzig's demonstration (39) in 1874 that lesions of the posterior portion of the dog's hemispheres produced blindness with paralytic dilatation of the pupil confirmed De Panizza's observations. Ferrier (24), about the same time, on the basis of ablations localized the cortical visual center in the angular gyrus of the monkey and in corresponding regions in dogs, cats, and rabbits. Munk (57) differentiated the visual zone which, when excised, gave rise to psychic blindness from the entire occipital lobe which, if destroyed, produced absolute and permanent loss of vision which he termed "cortical blindness." The validity of this concept and the anatomical areas involved were the subject of much discussion. Indeed, the matter has only recently been clarified by Klüver's (46) precise psychological studies which demonstrated the progressive encephalization of vision, with some subcortical discrimination of intensity but not form remaining in the higher primates.

However, Munk's theory of the topical projection of retinal segments upon the visual cortex and the corollary that in dogs blindness might be produced in any sector by extirpation of the corresponding cortical visual area, was indeed a shrewd hypothesis but one which required many more anatomical data than known then, for its proof. In the first place it required Minkowski's (54) demonstration, in 1911, that the visual cortical sphere coincided perfectly in the dog with the striate area and that the greater part of each retina is represented in the striate cortex of the oppo-

site occipital lobe and the small remaining part in the area striata of both sides. In the second place it required the carefully controlled experimental studies of Brouwer (10) and Polyak (59) to show the precise arrangements of the retinal projection. And finally it needed the clinicopathological material of Henschen (37), Monakow (55), Holmes (41), and others.

Frontal ablations. The portions of the cortex in front of the excitable motor area were likewise ablated in dog, cat, and monkey. Hitzig (39), Horsley and Schäfer (42), and Ferrier (24) all agreed that such excisions result in no motor or sensory paralysis. All noted a change in behavior and hyperactivity of the animal. Hitzig (39) described a "weak memory" for the animal would neglect food it had seen; Goltz (36) noted a stupid fixed expression on the face and a lack of normal fear. Bianchi (5) described the attention hemianopsia and its gradual regression. The behavioral disturbances following such ablations, although noted by the early investigators were described most precisely by Bianchi (5), later by Jacobsen and his associates and other members at Yale.

Central ablations. The excitability of the motor cortex attracted the attention of many investigators who sought the secret of its function through its excitation, in part or whole. The findings of Fritsch and Hitzig (29) were confirmed frequently in the latter part of the nineteenth century and interpreted as indicating a sensory motor function of this excitable cortex. Yet Munk (59) maintained that the entire cortex was sensory and the motor responses on stimulation were due to awakening of sensations which normally accompany such movements. On the contrary, Schäfer (66) believed that in the monkey there may be a paralysis of voluntary movement on one side without an appreciable loss of sensibility. Other observers took a compromising view, believing that the Rolandic cortex was sensori-motor. In the first half of this century this concept was reviewed by Dusser de Barenne (20) on the basis of strychninization of the cerebral cortex. But most neurophysiologists after the turn of the century have lost interest in this academic aspect and turned their attention to the accompaniments of the motor impairment, namely muscle tone, reflex changes, autonomic alterations, and patterns of the initial defect and its recovery. These aspects were discussed in the series of studies from Professor John Fulton's laboratory and most points have been argued in the pages of *Brain* and other neurological journals by Walshe (75), Denny-Brown (18), and others.

Temporal ablations. Ferrier (24) (1875) demonstrated that the first temporal convolution contained the cortical auditory area and reported that its destruction produced deafness. Other investigators disagreed, particularly with the latter statement. Munk (57) differentiated psychical deafness, which he considered to result from removal of the entire temporal lobes. The lack of intellectual deficits following partial cortical ablations was disconcerting to those who had considered the cerebral cortex the site of the intellect. Sänger-Brown and Schäfer (11) did report severe behavioral disturbances after removal of the temporal lobes, but little attention was directed to this finding at that time. Schäfer (67) states " it is in fact more easy to produce a condition of semi-idiocy in monkeys from extensive bilateral lesions of the temporal lobes, than from complete severance of the prefrontal region; an operation which may indeed be effected without producing any very obvious symptoms " (67, p. 901). It remained for Klüver and Bucy (47) to rediscover the marked behavioral changes before the subject was taken up with enthusiasm. Strangely, the auditory disturbances following bilateral temporal lobectomy were emphasized in neurological textbooks with neglect of the more behavioral alterations.

In this discussion the history of the concept of mind has been followed from the idea of a common pool of sensations mediating human conduct to the idea of highly organized cerebral centers, each representing a different function of the organism. This metamorphosis is a stage in the evolution of neurophysiology which was passed in the last century and which naturally led to the current stage when the attempt is being made to solve the problem of how these functional divisions of the cerebral hemispheres work together to make up that integration which we call the mind.

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JAMES MEASE, M. D., ON THE DISEASES OF DOMESTIC ANIMALS

J. F. SMITHCORS

The animal disease problem in America at the end of the eighteenth century was a disquieting one. No domestic animals had existed in America before livestock was brought to this country by the settlers, thus, unlike the situation in Europe, the livestock of the colonists was not threatened by a reservoir of disease-producing organisms. This deceptive immunity lasted for perhaps a century or more, but with the increase in numbers of livestock the inevitable happened, and a series of devastating outbreaks of disease made heavy inroads upon the animal population.[1] Having brought with them the traditional British indifference toward the practice of veterinary medicine, few men in agricultural pursuits had given much thought to the need for better knowledge of animal disease, or the means for promoting this knowledge.

In 1807, in response to a plea [2] made by the Philadelphia Society for Promoting Agriculture, Benjamin Rush had delivered an eloquent address upon "The Duties and Advantages of Studying the Diseases of Domestic Animals, and the Remedies Proper to Remove Them." (2) In this address to the students of the Medical School of the University of Pennsylvania, Rush had advocated greater attention to the study of animal disease on the part of physicians until more appropriate means of diffusing veterinary knowledge could be brought into being. One of the results of his lecture appears to have been "A Course of Lectures upon the Comparative Anatomy and the Diseases of Domestic Animals" by James Mease, M. D., who was secretary, later a vice-president, of the Philadelphia Society. The introductory lecture of Mease's course, which was given in the winter of 1812-1813, was published in the *Memoirs* of

[1] Rabies, first reported in 1770, was "very common" by 1779, and "raging" over the entire country in 1785. Dog distemper first appeared in 1760 with "great mortality." Horses and cattle "died in great numbers" in 1766; "nearly all" the young horses in New Jersey died of horse distemper in 1767. "Horn distemper" of cattle existed in epizootic form in Massachusetts from 1771-1785. In Philadelphia and New York, 9,000 cats died of cat distemper in 1798, and 80% of the chickens in Maryland died with gapeworms in the same year. (George Fleming: *Animal Plagues*, London, 1871.)

[2] "Essay and Plan for Promoting Veterinary Knowledge and Instruction. . . . Premium 9, 1806." *Memoirs Philadelphia Society for Promoting Agriculture*. 1: xxxvii 1808).

the Society for 1814 (*2*). The details of Mease's life, and his medical contributions, have been presented by Miller (3), who mentions, however, only the fact that Mease published his introductory lecture upon animal disease, and a number of "short papers on agricultural subjects."

Mease (1771-1846) was a younger contemporary of Benjamin Rush (1745-1813), and while the two were friends and co-members of the Philadelphia Society, Mease did not hesitate to differ with his illustrious colleague when the occasion arose. Mease's (4) concept of rabies in 1792 was far more advanced than that of Rush (5) in 1805. At the time, and for logical reasons, rabies was considered to be entirely a medical problem. Mease not only rejected the current concept of the spontaneous generation of the disease, but held advanced views on the contributions to the problem that might be made by a study of comparative pathology. On the other hand, Mease and Rush held almost identical views upon the need and means of increasing veterinary knowledge. Rush's manifold interests undoubtedly prevented his making more than passing observations upon animal disease. Mease, perhaps stimulated by Rush's plea for investigation of animal disease on the part of physicians, actively worked in the field of comparative pathology for more than thirty years. In addition to his contributions in this area, Mease also published papers in the Society *Memoirs* on such diverse subjects as: pine forestry, smut in wheat, manures for clover, varieties of wheat, and thorn hedges, but his contributions of greatest interest and value are those on animal disease.

In his introductory lecture upon comparative anatomy and diseases of animals, Mease (*2*) thinks it "very probable that a part of my hearers are entirely unacquainted with the subject upon which I am to lecture, and even with the meaning of the words '*Comparative Anatomy*.'" In explaining his subject, he says that one of his aims is "to point out its intimate connexion with Veterinary Medicine." The more widely read veterinary writers of the time had done little to enlighten their readers upon comparative anatomy; most of these works were little more than farriers' guides which placed primary emphasis upon cures.[3] Little or no thought was given to the anatomy of the principal subject, the horse, much less to physiology or pathology, or to the diseases of other animals.

[3] To 1813, only about two dozen works on animal disease had been published in America. Most of these were reprintings of British works, frankly labelled "Farriers' Guide," or the like. Only two were specifically identified as *veterinary* works: *A Dictionary of the Veterinary Art*, Philadelphia, 1805, by Thomas Boardman; and *A Compendium of Anatomy, Physiology, and Pathology of the Horse*, Philadelphia, 1806, by B. W. Burke. Both were British reprints.

That Mease intended to approach his subject in an entirely different manner, a manner largely unknown in America, is evident from his statement that,

> A distinction must be made between veterinary medicine and farriery. The first is founded upon science, whereas farriery disclaiming any connexion with science, proves itself a mere practice, habit or routine, and as it rests on nothing regular or solid, so it must ever be variable. The course of veterinary medicine and farriery are indeed the same, but with this difference, that the former condescends to admit a guide, while the latter prefers to ramble at risk and hazard.

Among the advantages which have resulted from a study of comparative anatomy, Mease mentions the elucidation of the structure and function of man, and the improvements in surgery resulting from experimental investigation. And,

> Comparative anatomy is as essential to the successful practice of veterinary medicine, as a knowledge of the structure of the human body is to the cure of the diseases and accidents incident to mankind. It is owing to a want of this knowledge of their structure, that our useful domestic animals are so mismanaged by farriers, and pretenders to animal medicine; and that diseases, trivial in their nature, or that slight surgical cases often end in death, or lameness, which might have been easily prevented by a scientific treatment.

Such ignorant practices as giving the horse when sick "the same drench from a horn, whether the disease be pleurisy or colic," will ". . . necessarily continue so long as veterinary medicine is not studied scientifically, or until medical gentlemen cease to think it beneath their notice; and I may add, until the owners of fine horses will by pecuniary rewards, encourage men of respectability and knowledge to engage in its practice." The proper plan of study, he adds, ". . . requires a leisure and education, far beyond the capacities and circumstances of those to whom the care of our animals has been hitherto abandoned, . . . we might suppose, that a greater degree of judgement and penetration are requisite for the physician of animals than of mankind."

As Mease points out, in ancient times "veterinary medicine was esteemed among the most important objects, and worthy of the consideration of an inquiring mind." Not only did eminent physicians of times past practice upon man and beast without discrimination, but later, when veterinary medicine had been far outstripped by its parent science, the leading medical minds were less apt to disparage the values of animal medicine than were the lesser lights in surgery and medicine. Even so, it was not until long after the establishment of schools of veterinary medicine in

late eighteenth-century Europe that public opinion toward veterinary medicine began to be more favorable. In America, little attention had been given to the necessity for veterinary education as the only proper means of elevating the practice of animal medicine, until men like Rush and Mease called for an enlightened view of the subject by medical men.[4] Recognizing that a separate system of veterinary schools could not come into being before there was a public acceptance of the distinction between farriery and veterinary science, both Rush and Mease urged the acquisition of veterinary knowledge by medical men.[5] Mease (2) says,

> If we consider the present state of animal medicine in this country, under its appellation of farriery, we see it in as deplorable a situation, as was the art of medicine, during the barbarous ages, when the gross ignorance of its professors brought disgrace upon the art itself, and when many diseases, which now yield readily to judicious treatment, raged without control; yet that the veterinary art, like human medicine, in the hands of a judicious person, is made respectable, we may see by the example of ancient times, and by the present example of several nations of Europe. . . .
>
> It remains for this country . . . to follow those examples; and by advancing the art to a height as yet unattained, to make amends for the neglect we have hitherto shown it. Indeed I am persuaded that in a short time the public attention will be called to the subject, and that men of education will think it no derogation from their medical character, to become acquainted with the diseases of cattle, or to lend their aid in the removal of them when required. . . .

In recommending the study of animal disease to the rural practitioner, Mease says this will not only be the means of "extending the sphere of his utility, but his personal consideration" as well. As a practical example of the debt of medicine to such study, he cites the case of John Hunter, who not only fostered the establishment of the Veterinary College of

[4] A similar approach had been used in Britain by John Hunter, who, as a leading proponent of the London Veterinary College (founded 1791), had stated, "The incompetence of persons to whom veterinary practice has been abandoned has drawn contempt upon the art, which should be second only to human medicine. . . . The idea must be abandoned that while medicine is the province of the learned few, anyone of ordinary capacity can acquire the veterinary art. . . . The nation requires a veterinary school in which the structure and diseases of animals can be scientifically taught." From *An Account of the Veterinary College from its Institution in 1791*. London, 1793.

[5] But Hunter had argued, "The arts of surgery and of human medicine can render very little service in cases of veterinary practice; while on the other hand, they may be, and frequently are, the source of error. . . . It requires the sacrifice of as many years to become a skilful veterinarian as to become a skilful physician; the acquisition of the science and the practice of each is a task sufficient to engage one man's life" (1793). Hindsight shows that Mease's plan might have borne fruit in America, for the few attempts to establish veterinary schools in this country before 1870 were unsuccessful.

London (1791), but also attended the practice of the college infirmary upon occasion. It was his observation of phlebitis following the bleeding of horses, and the dissection of the veins of affected subjects, that led Hunter to the seat of the condition in similar cases in man.

On the subject of rabies, Mease (4), who had in 1792 made the first real contribution to this problem in America, stated that "it still continues to humble the pride of the medical profession." Mease rejected all of the commonly accepted causes of rabies in the dog, particularly that of spontaneous generation and the redoubtable "worm under the tongue" (lyssa) claimed by innumerable authors since Pliny.[6] Concerning the latter cause, Mease states, "The idea of a worm is utterly false, no such thing exists." While Mease's statement leaves some doubt that he knew of the existence of the fibrous lyssa in the tip of the dog's tongue, the history of rabies would have been saner had this structure escaped notice altogether. Generations of hardy practitioners had removed the lyssa from healthy dogs as a preventive, and from rabid dogs as a "cure." Unlike some of his medical brethren who claimed to cure human hydrophobia, Mease (2) states emphatically that the disease has never been cured, but;

> We must not despair; for I cannot think that Providence has determined to permit this disease to be *forever* incurable, and can any more powerful argument be adduced for investigating the diseases of domestic animals, than the knowledge of this fact, that all of us are every day of our lives liable to the attack of an awful and incurable malady from one of them?

Mease gives a summary of the important outbreaks of epidemic diseases among domestic animals in America, and suggests that by a greater knowledge of them we might be led to anticipate fatal epidemics and provide the means of guarding against them.

There seems to be no record of the other lectures delivered by Mease, other than evidence that he did advertise that "lectures will be given every Tuesday, Thursday, and Saturday afternoon at half past three o'clock" (3) during the winter of 1812-1813. In his introductory lecture, he states that it is his intention to adopt the following plan:

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[6] Pliny says, "There is a certaine little worme in dogs tongues, called by a Greek name Lytta, which if it be taken out when they be young whelpes, they will never after prove mad." (*Historie of the Worlde*, Holland translation, London, 1634). Concerning the "worm," Samuel Johnson (*Dictionary*, 1755) says, "It is a substance, nobody knows what, extracted nobody knows why." According to George Fleming (*Rabies and Hydrophobia*, London, 1875), the practice of "worming" was still common in the late nineteenth century.

1. I shall demonstrate the structure of different animals.
2. Explain the use and functions of the several parts, and compare them with those of the human body.
3. Point out the causes, nature and symptoms of diseases in our domestic animals, with the method of cure.
4. Give the natural history, operations and doses of medicines.

In summary, he says,

From this plan it will be seen, that farriery, strictly so called, or what relates to the fashionable operations on a horse, makes no part of the course. By thus separating the scientific from the merely mechanical part, the veterinary science will be at once put in a condition to go hand in hand with human medicine.

Mease recognizes the propriety of teaching the proper methods of shoeing the horse, but his insistence upon the separation of the scientific from the mechanical aspects of the art was largely forgotten by later veterinary educators until long after the heyday of the horse.[7] I recall spending long hours over a forge beating out a horseshoe from a bar of iron, but hearing nothing about the newly discovered antibiotics. Fortunately, such a situation no longer prevails.

What the immediate effects of Mease's lectures may have been is difficult to determine. Certainly they were the first efforts in this direction in America. There would seem to be little doubt that his lectures stimulated a continuing interest in animal diseases on the part of members of the Philadelphia Society. Inasmuch as practically all of the early promoters of a veterinary profession in America were associated in some way or another with the Society, it would seem a safe assumption that Mease's lectures had some influence upon veterinary perspective. In 1837, Peter A. Browne, a lawyer, and professor of geology at Lafayette College, addressed the Philadelphia Society on the "Veterinary Art."[8] Upon the establishment of the first veterinary school in America, the Veterinary College of Philadelphia in 1850, the Society granted the ill-fated college the use of its rooms for lectures. In addition, the Society invited the

[7] Long after their establishment in both Britain and America, veterinary colleges were little more than formal schools of farriery. In Britain, Granville Penn, a relative of William Penn, in proposing a scientific veterinary college, stated, "The very name of 'farrier' must not be mildly excised and cleansed . . . but altogether expunged. If it remains it must divide itself from science, as astrology from astronomy. . . ." (*Gentlemen's Magazine*, *60*: 1071, 1790). Yet the first professor of the London school, Charles Vial de Sainbel, chose to entitle a text for students, *Lectures on the Elements of Farriery*. (London, 1793.)

[8] Bierer (8), page 34. Browne's address was published in a 22 page booklet, and reprinted in *The Cultivator* (New York) *5*: 11, 1838.

faculty of the college to deliver a series of lectures on hippology under its auspices.

In his introductory lecture of 1813, Mease mentioned the fact that he had investigated a fatal cattle disease in 1796, the particulars of which, "I shall hereafter detail." The one fact, and an important one, he mentions in the body of his lectures is that the reservoir of the disease was a small area of South Carolina, and that apparently healthy cattle from this area infected all others with which they mixed in being driven north. He says: "A singular fact attending this disease is, that the cattle alluded to, have the power of infecting others with which they associate, while they themselves are in perfect health." The existence of the disease, later designated as Texas fever (now piroplasmosis) had been reported as early as the 1790's,[9] but Mease's investigation of 1796 and his report of 1814 are the first scientific contributions on the subject. While Mease did not identify the tick as a vector in the spread of the disease, his identification of immune carrier cattle was largely forgotten until Theobald Smith and his associates began a frontal attack on the problem in the 1880's. In the open-minded spirit characteristic of the true investigator, Mease did not discount the value of observations made by those closest to the problem; his identification of immune carriers was based upon information which had been given him "by an intelligent drover." In a similar vein it was the faith of the veterinarian, Cooper Curtice, in cattlemen's observations on the tick, that led him, in spite of professional ridicule, to the final incrimination of the tick in the 1890's.

In his article of 1826 on "An Account of a Contagious Disease Propagated by a Drove of Southern Cattle in Perfect Health" (6), Mease reported his earlier observations on symptoms and epidemiology in great detail, thus providing an invaluable first chapter in the written history of Texas fever. By this time the disease was widespread in eastern Pennsylvania, and was commonly denoted as the "bloody murrain" inasmuch as "some discharged bloody urine, others bled at the nose." Mease continues,

> Upon being opened, the kidneys were found inflamed, and sometimes in a state of suppuration, and the intestines filled with hard balls. I prescribed strong purgatives. To one I gave two ounces of calomel, in sweet oil, on the second day of the disease, but without producing any evacuation. Bleeding was tried, without success. The blood was in a state of decomposition, and did not coagulate. As a

[9] "In 1766 the Colony of North Carolina passed laws to control the movement of cattle from South Carolina to Georgia." (Bierer (8), page 49.)

preventative I recommended smearing the nose, horns, forehead, hoofs, and tail with tar, to counteract the contagion of the disease, by creating an artificial atmosphere around the animal, and also the obvious expedient of an entire separation of the old stock from the strangers. None of the southern cattle died. . . .

The useful deduction of which the foregoing statement admits, is a caution in respect to the mixing northern and southern cattle, without the performance of a kind of quarantine by a strange drove, before they are permitted to associate with the stock already on the farm.

Mease makes it clear that the southern cattle were confined "for one night in a ploughed field," and that the northern cattle which became affected "had no intercourse with the drove." With additional observations of similar instances, it might be supposed that a logical thinker like Mease could have arrived at the deduction that some intermediary agent, in this case the cattle tick, must have been operative in producing the contagion. At this time, however, the tick had not been recognized as more than a minor depredator in its own right. In 1868, after cattlemen had begun to suspect some association of the disease with the tick, the eminent Briton, John Gamgee, V. S., M. D., was invited to investigate the disease then known as Texas fever. After an exhaustive study, Gamgee (7) listed a number of possible causes,[10] all wrong, but ridiculed the tick theory, saying, "A little thought should have satisfied anyone of the absurdity of the idea." If, as Bierer (8) suggests, "revelations of the kind necessary to establish the cause of Texas fever just weren't meant for the year 1868," it is understandable that the true nature of the contagion should have eluded Mease in 1796. Bierer continues, "If Gamgee had advanced the tick theory, he would probably have been the laughing stock of the scientific world. It was all the scientists of the United States Bureau of Animal Industry could do, decades later, to put the idea across after years of careful research." [11]

In the absence of knowledge of the real cause of the disease, it is not surprising that Mease's treatment of affected animals, although based upon accurate post-mortem findings, would avail little, particularly in arresting the contagion. As health officer of the Port of Philadelphia, however, Mease had demonstrated sound principles of quarantine in dealing with yellow fever; segregation similar to but more stringent [12] than

[10] Including excreta, soil, vegetation, feed, and water.

[11] Bierer (8), page 53.

[12] "Cattlemen of the affected states organized; patrols were appointed in southern Missouri with orders to turn back all Texas droves because they 'were subject to an epidemic or contagion somewhat resembling the yellow fever of the human race.'" (Bierer (8), page 51; quoted from *Eighth Census of the United States: 1860.*)

that suggested by Mease was the only effective means of arresting the spread of Texas fever before the role of the tick was fully elucidated.

In addition to having made the first scientific observations in this country upon rabies and Texas fever, Mease also may be credited with being the first American to identify ergot poisoning among domestic animals. While Mease (9) in his report of 1826 "On the Hoof Disease from Eating Hay Affected with Ergot" credits a Dr. Arnell with having made a similar observation in 1820, Mease's own observations go back to 1803. He states that cattle which ate hay infested with ergot "became affected with a disease in their hoofs, causing them sometimes to drop off." Concerning Arnell's description of the disease, Mease says: The facts detailed by him, leave no doubt of the deaths of numerous cattle in his vicinity being caused by their eating hay made from some grass that was affected with the species of ergot, observed in the produce of the meadows before mentioned.

Had the observations of James Mease been made during a time of great activity in the field of veterinary investigation, not only would he still have been a giant among men, but his contributions would have received just recognition. Because of the lack of interest in animal disease during Mease's lifetime, and for some decades later, no appropriate vehicle for the reporting of observations upon animal disease existed. Due largely, perhaps, to the increasingly metropolitan character of the City of Philadelphia, publication of the *Memoirs of the Philadelphia Society for Promoting Agriculture* was suspended with the volume of 1826. Thus the veterinary contributions of Mease were buried in a once respected, but later little remembered tomb, until they were resurrected recently by Bierer (8) in his *Short History of Veterinary Medicine in America.*

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JOHN MITCHELL, BENJAMIN RUSH, AND YELLOW FEVER *

SAUL JARCHO

In his famous *Account of the Bilious Remitting Yellow Fever, as it Appeared in the City of Philadelphia in the Year 1793* [1] Benjamin Rush said:

> Baffled in every attempt to stop the ravages of this fever, I anticipated all the numerous and complicated distresses in our city, which pestilential diseases have so often produced in other countries. . . . I ransacked my library, and pored over every book that treated of the yellow fever. The result of my researches for a while was fruitless. . . . Before I desisted from the inquiry to which I had devoted myself, I recollected that I had among some old papers, a manuscript account of the yellow fever as it prevailed in the year 1741, which had been put into my hands by Dr. Franklin, a short time before his death. I had read it formerly, and made extracts from it into my lectures upon that disorder. I now read it a second time. I paused upon every sentence; even words in some places arrested and fixed my attention. . . .

At this point in his text Rush inserted a long excerpt from John Mitchell's manuscript advocating purgation in acute fevers. Rush then continued: "Dr. Mitchell in a moment dissipated my ignorance and fears upon this subject. I adopted his theory, and practice, and resolved to follow them. . . ."

And so, with the enthusiasm of a religious convert who has just enjoyed a transcendental vision, Rush hurried away to deplete Philadelphia.

In view of Rush's statement it is clearly our obligation to examine the experience of John Mitchell. This is recorded in the aforementioned manuscript, which forms part of a complicated correspondence that ultimately came to involve Mitchell, Franklin, Colden, Rush, and Hosack. The labyrinthine vicissitudes of these papers have been described briefly in another publication.[2] The present study is limited to an analysis of John Mitchell's first-hand experience with the disease.

* Read in part at the twenty-ninth annual meeting of the American Association of the History of Medicine, Durham, N. C., April 20, 1956.

[1] Philadelphia, Dobson, 1794, pp. 196 ff.

[2] Jarcho, S.: Cadwallader Colden as a Student of Infectious Disease. *Bull. Hist. Med.* *29*: 99-115, 1955. See esp. pp. 103-106, and notes 40, 41, 44, and 45 on p. 114. See also

Mitchell's observations were made at Urbanna on the Rappahannock River in Middlesex County, Virginia, at a latitude of approximately 37° 30′. Mitchell nowhere states clearly how many cases occurred or how many he had seen. The only specific mentions are as follows:

1. an autopsied case in a slave dead of yellow fever, *February* 14, 1741/2.[3]
2. an autopsied case, *February* 13, 1741/2.[4]
3. a patient who was bled on *February* 17, 1741/2.[5]
4. and 5. two autopsied cases in the *spring* of 1737.[6]
6. an autopsied case in *September*, 1742.[7]

In addition Mitchell states that yellow fever occurs chiefly in winter and spring,[8] from Christmas to Whitsuntide.[9] The disease is remarkably contagious, but spreads more slowly than measles or smallpox. It spreads fastest in the spring.[10] It has twice been brought to Virginia by His Majesty's ships of war.[11] Relapses may occur.[12]

The evidence presented by Mitchell thus includes mention of six cases. Detailed histories of individual patients are not given but the generalized clinical picture is said to comprise six symptoms:[13]

a. a very great and sudden debility, without any manifest cause
b. a feverish anxiety, generally very grievous
c. short quick difficult respiration, after the fever is formed
d. a contracted deep pulse
e. pain in the scrobiculus cordis
f. a yellowness in the eyes or all over the body, to which may be added a violent and unusual headache.

One of Mitchell's six patients is noted to have died on the fourth day, another "on the sixth or seventh day."

With regard to the clinical description—or, more accurately, the clinical summary—given by Mitchell, we must admit that it contains nothing

Packard, F. R.: The Manuscript of Dr. John Mitchell's Account of the Yellow Fever in Virginia. *Proc. Charaka Club* 9: 44-46, 1938.

[2] Mitchell, John: Account of the Yellow Fever which prevailed in Virginia in the years 1737, 1741, and 1742. *Amer. Med. and Phil. Register* 4: 181-215, 1814; see p. 183.

[3] *Ibid.*, p. 186.
[4] *Ibid.*, p. 188.
[5] *Ibid.*, p. 187.
[6] *Ibid.*, p. 187.
[7] *Ibid.*, p. 194.
[8] *Ibid.*, p. 195.
[9] *Ibid.*, p. 195.
[10] *Ibid.*, p. 197.
[11] *Ibid.*, p. 203.
[12] *Ibid.*, p. 183.

which is positively inconsistent with later clinical descriptions of yellow fever. At the same time it is important to remember that the clinical diagnosis of yellow fever is not easy, especially when cases are few. Indeed Manson-Bahr remarks that " Severe yellow fever may be confused with remittent subtertian malaria, Weil's disease, infective hepatitis, relapsing fever, and blackwater fever. The difficulties of clinical diagnosis are often great, especially early in an epidemic." [14]

If we momentarily limit our attention to the clinical portrayal, excluding all else, we must grant that Mitchell's patients *may* have had yellow fever. When we turn to the epidemiologic facts, however, a different conclusion forces itself upon us.

Yellow fever in the United States is a sharply characterized disease, which produced a constant epidemiologic pattern through the eighteenth, nineteenth, and early twentieth centuries. The disease was regularly conveyed to this country by ships. The first cases most often appeared about the middle of July or later, during the hot part of the year. After the initial infections the incidence would rise slowly, then rapidly. The disease would prevail thereafter until the cold weather of autumn, which regularly ended the outbreak.

The seasonal character of yellow fever in the United States is strongly evident in the encyclopedic compilation of August Hirsch.[15] A table in Hirsch's chapter on yellow fever shows that no outbreaks ever occurred in the United States in the first four months of the year, and that no outbreaks had ever been recorded from Virginia and adjacent states earlier than June. With this in mind, if we return to Mitchell's cases, we observe that three occurred in *February* 1741/2, two occurred in the *spring* of 1737, and one in *September* 1742. Hence of the entire series of six cases only the last could very well have been a case of epidemic yellow fever.

Only one epidemiologic detail in Mitchell's essay suggests the possibility of yellow fever. This is the contention [16] that the disease had been brought to Virginia by ships. However, in a letter to Cadwallader Colden [17] dated September 10, 1745, Mitchell wrote:

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[14] Manson-Bahr, P. H.: *Manson's Tropical Diseases*. Thirteenth edition, Baltimore, Williams and Wilkins, 1950, p. 368.

[15] Hirsch, August: *Handbook of Geographical and Historical Pathology*. London, New Sydenham Society, 1883-1886, vol. 1, p. 350.

[16] Mitchell, *op. cit.*, p. 197.

[17] *The Letters and Papers of Cadwallader Colden*, vol. 8, p. 326. New York, New-York Historical Society, 1937.

. . . I have been very careful to trace its origin, no less than four times among us; & find that it was allwaies imported in British vessels from England, & once in a man of War, that came directly from London, after a long & tedious voyage.

Importation directly *from England*, especially in a ship which had had a long and tedious voyage, certainly does not support the diagnosis of yellow fever.

We may therefore conclude that at least five of Mitchell's six cases were not cases of yellow fever. Can we hazard a guess as to what they were? At least three possibilities come to mind—leptospirosis, severe infectious hepatitis, and pneumonia complicated by icterus. With the scanty evidence available to us a reliable decision is probably impossible. We can at least state that the epidemiologic facts are consistent with what is known about infectious hepatitis. Havens and Paul [18] say the following:

In certain parts of the world there is a distinct seasonal trend, with a sudden increase in prevalence in the autumn, often building up to epidemic proportions during the early winter and declining in the spring. However, the epidemic season may reach a peak in late winter and extend well into the spring.

In the foregoing discussion it is not desired to imply that John Mitchell was the only source of Rush's ideas about the treatment of yellow fever. Mitchell contributed an elaborate rationale for a treatment which consisted chiefly of sweating and purgation. This system Rush put into effect by applying, in greatly augmented doses, a regimen of calomel and jalap concocted during the Revolutionary War by Thomas Young [19] for the treatment of bilious (typhoid) fever. The addition of phlebotomy, which Mitchell did not warmly advocate in yellow fever, depended on other complex theoretical considerations and precedents.[20]

If we attempt to view the work of Mitchell and Rush in historical perspective, two inferences become apparent. The first has to do with the general nature of disease, the second has to do with Benjamin Rush personally.

It is well recognized that a given disease such as yellow fever is an abstract concept derived from the primary data, i. e., from clinical and

[18] Havens, W. P., Jr. and Paul, John R.: Infectious Hepatitis and Serum Hepatitis, *in* Rivers, T. M.: *Viral and Rickettsial Infections of Man.* Philadelphia, Lippincott, 1948, p. 275.

[19] Rush, *op. cit.* (see footnote 1), p. 200 ff.

[20] Rush, *ibid.*, p. 258 ff. See also Butterfield, L. H. (editor): *Letters of Benjamin Rush.* Princeton Univ. Press, 1951, pp. 695, 815, 822. See also Rush's Medical Theories, *in* Corner, George W.: *The Autobiography of Benjamin Rush.* Princeton Univ. Press, 1948, pp. 361-366.

laboratory observations. These abstract concepts necessarily evolve. In the middle of the eighteenth century the abstract concept of yellow fever was incompletely and imperfectly separable from the concept of other febrile diseases in which jaundice occurs. Mature differentiation did not develop until the isolation of the causative virus was accomplished by Stokes, Bauer, and Hudson in 1928. Further evolution occurred in the 1930's with the elaboration of the concept of jungle yellow fever. The work of Mitchell and Rush thus represents a crude early stage in a long process.

A second inference concerns Benjamin Rush himself. According to his own testimony he ransacked his library, found the Mitchell manuscript, and immediately accepted its contentions. In the urgency of the growing outbreak he never paused to consider whether the disease described by Mitchell really was yellow fever. The self-portrait which Rush gives is one of a quick impulsive type, a doer and not a thinker. This is the man who armed himself with John Mitchell's questionable diagnosis and went off to kill the dragon of yellow fever.

BENJAMIN FRANKLIN AND THE RISE OF FREE TREATMENT OF THE POOR BY THE MEDICAL PROFESSION OF PHILADELPHIA *

ROBERT J. HUNTER

In 1750 a Philadelphia doctor came to Benjamin Franklin to ask help in raising funds for a new hospital. Franklin was enthusiastic and organized a successful drive, resulting in the founding of the Pennsylvania Hospital on May 11, 1751. The doctor offered to serve gratis, for three years, on the medical staff. Thus arose, in Philadelphia, a custom which spread through the United States and has continued to this present day in 1956. Benjamin Franklin had some influence on this rise of free hospital care of the sick poor by the medical profession. It is appropriate that we discuss this accomplishment of the Great American on the 250th anniversary of his birth in 1706.

Even before the Pennsylvania Hospital was created, doctors were on duty at the Almshouse in Philadelphia [1] and the Province Island Hospital service in Philadelphia, receiving salaries for their services, and the Almshouse (founded on May 10, 1729) was, despite its name, already a hospital in every sense meant when we speak of hospitals before the 18th century: an institution where, among other benefits, people with many kinds of illness were receiving medical care from a doctor and where sick people were admitted. The Almshouse has grown into a great and famous hospital, The Philadelphia General Hospital, Blockley Division. But the Pennsylvania Hospital which Franklin founded and called the first in Philadelphia was planned to be of that *new type* of hospital which had begun to emerge in the period when new ideas of the rights of man were beginning to be put into effect. We can call it the first hospital in Philadelphia only if we think hospitals never existed anywhere in the world until shortly before it was founded.

* Read at the twenty-ninth annual meeting of the American Association of the History of Medicine, Durham, North Carolina, April 21, 1956, and at the 250th Anniversary of the Birth of Benjamin Franklin, at the Philadelphia General Hospital, Blockley Division, Philadelphia, Pa., October 18, 1956.

[1] This institution has had various names as follows: The Philadelphia Almshouse, The Philadelphia Almshouse and House of Employment (The Bettering House), The Philadelphia Almshouse and Hospital (Blockley), The Philadelphia Hospital, The Philadelphia General Hospital, The Philadelphia General Hospital (Blockley Division).

What we wish to emphasize now is that the planning of the Pennsylvania Hospital reflected *an important change in philosophical viewpoint* over that of earlier ages. Let us examine what Franklin has to say in his *Autobiography*,[2] his *Some Account* [3] and in the *Petition to the House*, which is said to have been written by him but not signed, and in other documents quoted in Morton's *History of the Pennsylvania Hospital*.[4] We can tell at once that Franklin knew, intimately, the rules and regulations pertaining to the care of the sick poor in Philadelphia before 1750. Both the name " Pennsylvania " chosen for the hospital and the phrases " when the poor came to town," " even the poor inhabitants of the city, tho they had homes," " poor sick persons whether inhabitants of the province or strangers," and " kind care of sick and distempered Strangers . . . given heretofore . . . think you will give an equal concern for the Inhabitants," show us that the system Franklin was proposing was to be *an extension to a much wider group*, of the medical care already being given to the sick poor on the parish or township level by the Philadelphia Almshouse and to incoming ships' passengers from abroad by the hospital on Province Island, which had been built in 1743 to provide a quarantine service. The restrictions demanding residence and pauperism, which were conditions *sine qua non* for admission to the Almshouse, were to be removed in the case of the new Pennsylvania Hospital; local inhabitants other than paupers, and strangers as well, were now to be eligible for hospital care, partly paid for by private benevolence replacing the poor tax that had been the prevailing custom and the alms of former monastic days.

We now come to an episode which, though it may have occurred spontaneously, really stems from the oath of Hippocrates and the very foundations of the priest-physician concept of the duties of a physician. Three doctors, leaders in their community and men of great benevolence, stepped forward and offered to serve free of charge for three years. We will quote this passage in full. In the " Brief Account " or *Some Account*, Franklin says: [5]

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[2] Benjamin Franklin, *Autobiography*, Edited by John Bigelow, J. B. Lippincott and Co., Philadelphia, 1868. This edition includes " The Story of a Famous Book. An Account of Dr. Benjamin Franklin's Autobiography," by Samuel A. Green, M. D.

[3] Benjamin Franklin, *Some Account of the Pennsylvania Hospital, From Its First Rise, to the Beginning of the Fifth Month, Called May, 1754*. Printed by B. Franklin and D. Hall, Philadelphia, 1754.

[4] Thomas G. Morton and Frank Woodbury, *The History of the Pennsylvania Hospital, 1751-1895*. Philadelphia, Pa., 1897, p. 8.

[5] *Loc. cit.* (footnote 3), p. 9.

On the second reading of the Petition, January 29, 1751, the House gave leave to the Petitioners to bring in a Bill, which was read the First Time on the first of February. For some time it was doubtful whether the Bill would not miscarry, many of the Members not readily conceiving the Necessity or Usefulness of the design; and apprehending moreover, that the Expense of paying Physicians and Surgeons, would eat up the whole of any Fund that could be reasonably expected to be raised; but three members of the Medical Profession, vis.: Doctors Lloyd Zachary, Thomas Bond, and Phineas Bond, generously offering to attend the Hospital gratis for three years, and the other Objections being by degrees got over, the Bill, on the seventh of the same Month, passed the House, *Nemine Contradicente*, and on the 11th of May following, it received the Governor's approval.

We can see by the context that service gratis by the doctors was not the original plan but that it was put in because, as Franklin indicated, it looked as if the bill could not pass. This offer to serve without remuneration was most unusual since it was the custom of that day to pay salaries to doctors for medical work performed in public positions. That the members of the Assembly expected a fee would be paid is indicated by their objection to the cost of the new venture. I cannot give documentary proof of Franklin's knowledge of the fact that a salary was paid to the doctor at the Almshouse, but it is very reasonable to assume that with his active interest in all affairs that went on about him, he did know that the doctor was paid a yearly salary of £30 in 1740 and £50 in 1751 (circa).[6] Franklin's shop near Front and Market Streets was but a few blocks from Third and Spruce Streets where the Almshouse occupied an entire city block. He was well acquainted with the doctor on duty there, William Shippen, Sr., who was a member of the American Philosophical Society, which Franklin founded. Franklin and the doctor's nephew, Edward Shippen, were appointed Justices of the Peace in 1749.[7] Franklin also was intimate with Dr. Thomas Bond, Sr. who had come to him for help in planning the hospital. They were members of the same Masonic Lodge, the American Philosophical Society, the Board of Trustees of the Academy, and had served together on the City Council. It is not unlikely that Franklin also knew that Dr. Bond was appointed Port Physician in 1741, taking the place of Dr. Thomas Graeme who resigned after holding the position for twenty years. The salary was one pistole a day. We have no record of Bond's resignation. It may be that he was still serving in 1751. Moreover, Dr. Bond was not averse to receiving a salary some years

[6] Robert J. Hunter, *The Origin of the Philadelphia General Hospital, Blockley Division.* Philadelphia, Pa., 1955, p. 13.

[7] John Hill Martin, *Martin's Bench and Bar of Philadelphia.* Philadelphia, 1883, p. 44.

later than 1751, for the manuscript records of the Philadelphia Almshouse, which are at the Philadelphia General Hospital, having come down to us through almost two hundred years, note that he was paid £50 a year from 1768 to 1779 when he retired from the Almshouse staff at the age of sixty-seven. Lloyd Zachary,[8] another of the three physicians who volunteered to serve gratis at the new hospital, had also been, and may even have been at that time, a salaried public servant, having served as a port physician for at least 12 years and possibly even longer. The salaries these men received were good, for those days. The medical work at Province Island, which Bond and Zachary did, was intermittent. It entailed a full day's ride on horseback or a day's sail on the Delaware River, to reach the island and to return. They inspected "sickly vessels" and the passengers, mostly immigrant, arriving on them. This quarantine service, to protect the public health, has continued even to the present day and always with a salary for the physicians. As to the Almshouse position, all Dr. William Shippen, Sr. had to do when treating patients there was to walk over from his drug store to Third and Spruce Streets and give an order for some saltpeter, senegal snakeroot for the rheumatism, a bottle of Godfrey's Cordial for McConnel's child [9] and, leaving all secure, return home on a walk through "Society Hill."

In view of these facts one might suppose that Dr. Zachary and the Doctors Bond anticipated that the new hospital might eventually pay salaries similar to those then in vogue in the city. That they were expecting some fee after the new hospital became established is suggested by the fact that they limited their original offer to the first three years. On the other hand we have no word of any discussion taking place between Franklin and Bond on the question of fees for the doctors. Franklin, in telling how he influenced the Assemblymen to vote favorably on the Petition for the hospital, says, "I more easily excused myself for having made some use of cunning." [10] It is not impossible that he also artfully pointed out to Bond some of the emoluments that might accrue from a hospital position, in the way of private practice. I think appointment to a hospital staff is still a potent factor in the success of a doctor. It is an established fact that appointment to the new hospital did prove to be lucrative. Presently we find records of medical men serving concurrently on the staffs

[8] Edward W. Mumford, "The Birth of a Great University," *The Pennsylvania Gazette*, vol. 33, no. 8, Jan. 15, 1935, p. 224. Franklin is quoted as saying of Zachary, "In sweetness of Temper, politeness of manners and universal Benevolence, he had few Equals and no Superiors."

[9] These were ordered by Dr. Shippen in 1767.

[10] *Loc. cit.* (footnote 2), p. 281.

of the Almshouse and the Pennsylvania Hospital and also on the faculty of the Medical School of the College of Philadelphia (subsequently the University of Pennsylvania) founded in 1765. They were paid a salary at the Almshouse where their patients were paupers and were satisfied with the prestige at the other two institutions, which brought contacts not found among the paupers. Although Dr. Thomas Bond, Sr. was never formally elected to the faculty of the College we find him giving the introductory clinical lecture at his home and a course in obstetrics at the Philadelphia Almshouse. Each student had to pay a guinea to each professor on taking his degree of Bachelor of Physic. The price of tickets for the course was not to exceed six pistoles ($20).[11] Franklin was president of the Board of Trustees, but not too active because he was out of the country so much and also because he did not like the provost. Of the original faculty all but Morgan and Shippen, Jr. received a salary from the Almshouse at one time or another. It is not necessary, however, to cast about for any other reason for the remarkable step of serving gratis at the inception of the Pennsylvania Hospital than to know the benevolent character of the men who made the offer, and the spirit of their times. We can find precedent for it in the philosophy of 17th century England and in pre-revolutionary France.[12] Having taken the step, the staff of the Pennsylvania Hospital went on giving its medical services without salary, as it has continued to do right up to the present time.

Shortly after the original offer of the physicians to serve without pay, the Contributors to the Pennsylvania Hospital passed a rule that the managers should serve without any fee. They were to relinquish any "claim to receive or retain any fee Gratuity or reward whatsoever." [13] In 1763 the medical staff gave further evidence of their generous intent by proposing to turn over to the managers for the founding of a medical library in the hospital such gratuities as were paid by the students attending the hospital wards; gratuities which custom had dictated to be the right of the physician to appropriate.[14] In 1767 Dr. Thomas Bond is recorded as turning over to the managers cash received from students in his clinical lectures.[15] In addition Dr. Bond probably received fees from his private students or apprentices,[16] as was the custom in England, and from

[11] Francis Newton Thorpe, *Benjamin Franklin and the University of Pennsylvania.* Washington, D. C., 1893, p. 273.

[12] George Rosen, "Medical Care and Social Policy in Seventeenth Century England." *Bull of the N. Y. Acad. of Med.* Second series, vol. 29, no. 5, May 1953, *passim.*

[13] *Loc. cit.* (footnote 4), p. 30.

[14] *Ibid.*, p. 346.

[15] *Ibid.*, p. 469.

[16] *Ibid.*, p. 471.

pupils in his obstetrics class at the Almshouse. It is said somewhere that he also turned over the fees from the Almshouse students. The atmosphere in which these actions were carried out is shown in a letter Franklin wrote to a friend twenty-three years after his work in establishing the Pennsylvania Hospital. He was able to boast how the Committee of Safety in Philadelphia, appointed by the Assembly, "attend closely, without being bribed to it, by either salary, place or pension or any hopes of any, which I mention for your Reflection on the difference, between a new virtuous People, who have publick Spirit, and an old corrupt one, who have not so much as an Idea that such exists in Nature." [17]

In 1783 another important episode occurred. It set the pattern, still prevailing in hospitals in the United States today, that the Board of Directors has *the authority to control medical fees which the doctors charge in the hospital.* A brief outline of what took place is as follows: the managers of the Almshouse had made arrangements with the managers of the Pennsylvania Hospital to have venereal patients treated at the hospital for a set fee. When the bill was presented to the managers of the Almshouse a charge had been added for fees for two doctors. The following are some casual abstracts from "The Minutes of the Managers of the Philadelphia Almshouse and House of Employment": [18]

Feb 7 1783. Col Henry and Mr. Rush are desired to meet the Managers of the Hospital to confer with them respecting the charge of their physicians for attending sundry of their poor patients for whom we pay them weekly Board and expected there was to have been no further charge, to endeavor to get the poor patients admitted there on Charity as formerly or to have the prices lowered, some being now charged at £15 per Ann., while others in the same circumstances are at 10 shillings per week, also to Request that our Physician may be permitted to attend the poor Patients there, rather than to submit to their charges. An account from the managers of the Hospital then being presented, our President was requested to draw an order on the Treasurer, for the amount of the Hospital charges, but not for the Doctors, the total amount being £75/15/0. Deduct £10/10/0 Physicians charges leaving £65/5/0 to be paid.

The committee reported on May 7th that they had not yet met the managers of the hospital and similarly at a later date, and finally that the matter was settled. No mention is made of the names of the doctors involved. Apparently the managers of both hospitals acted only after

[17] Leonard W. Labaree and Whitfield J. Bell, Jr., *Mr. Franklin. A Selection from Personal Letters.* Yale University Press, 1956. "Letter to Jonathan Shipley, Philadelphia, July 7, 1775," p. 30.

[18] Manuscript copy at The Osler Memorial and Blockley Historical Museum, Phila. Gen. Hosp. (Blockley Div.).

careful deliberation. The other side of the picture is given in "The Minutes of the Managers of the Pennsylvania Hospital:[19]

3rd of 3rd Mo 1783. William Rush, William Henry and George Emlen, a Committee from the Managers of the House of Employment, applied to know whether the Board approve of the physicians of the Hospital charging them with three pounds 10/ a fee for curing Patients under the venereal Disease, and sent here by the Overseers of the Poor—the Board told them it should be enquired into, wherefor it is agreed that the Sitting Managers confer with the Physicians thereon, and report to the next meeting. . . .

31st of the 3rd Month 1783. . . .

28th of the 4th Mo 1783. . . . The Monthly Committee Report, that they have conferred with the attending Physicians, on the complaint made by the Managers of the House of Employment, against the fee of three pounds ten shillings, charged in venereal Cases by two of the Doctors which being again taken into Consideration, the Managers, Resolve, that in such cases the Physicians of the House are not entitled to, and therefore should not demand any fees, or reward for their services.

12th of 5th Mo 1783. Doctors Thos. Bond, John Morgan, Thomas Parke, James Hutchinson, John Jones and Adam Kuhn are unanimously elected.

Dr. John Morgan replied to this decision with a long letter as follows:[20]

Phila., May 24, 1783.

To the Managers of the Pennsylvania Hospital:

Gentlemen . . . every real object of Charity had an undoubted claim, and most affectionate welcome from me.

But I beg leave to remark that in whatever light I view the resolution of your board on the 28th ulto. in respect to certain Almshouse Patients (I speak it with the utmost deference to the Purity of Intentions and Supreme Wisdom of the Managers) it appears to me to be both impolitic and injurious to the original design of this excellent Charity, as well as incongruous with the Sentiments of the Founders, and especially with the first Physicians of the Hospital, which I always supposed were to perform Acts of Charity to Objects and Cases of Charity. I never imagine they meant to subject themselves or their Colleagues and Successors in office, to the Extra-Jurisdiction of men, whether in public or private stations, who have no authority over them; not [nor] that they came under any obligation to devote their time and attention to the Cure of Diseases brought on by Concupiscence, without fee or reward, which in my humble opinion tends rather to the growth than diminution of Immorality. If I am not misinformed that resolution took its rise from persons not belonging to your Board, who have no claim to the Services of the Hospital Physicians in behalf of those committed to their charge; and who have and exercise the power of nominating, and of paying a Physician for that Duty, whose place thereby becomes a lucrative Sinecure; and

[19] Manuscript copy at The Historical Library and Museum, Pennsylvania Hospital, Phila., p. 290.

[20] *Loc. cit.* (footnote 4), p. 468.

whilst one set of Gentlemen perform the Services, without thanks, emolument or honor, another reaps the fruit of their labors.

That this regulation should be adopted on taking only the opinion of the two attending Physicians, without any regard paid to the judgement of the rest, on what regards the honor and interest of each other, present and future, equally with theirs, will not permit me, consistently with my sense of propriety, to act longer in that Station. In any Institution that looks for support from all, every Physician has an equal right to be consulted in regards the Faculty.

. . . , I readily resign my place to others whose views are more conformable to your sentiments.

. . . I remain Gentlemen, your respected sic. Friend and Fellow-Citizen,

John Morgan

"The Minutes of the Managers of the Pennsylvania Hospital": [21]

26th of 5th Mo 1783. Dr. John Morgan by his Letter of the 24th Instant, declining to continue one of the Physicians of this House, the Managers appoint Peter Reeve, and Isaac Wharton to thank him in the name of the Board for his service he has rendered to this Institution.

This episode of Dr. Morgan's resignation from the Pennsylvania Hospital following a dispute about a charge he had made for patients sent there for special treatment by the managers of the Philadelphia Almshouse and House of Employment must be considered in the light of the fact that the war had just ended. Inflation was rife. The managers of both institutions were in a quandary. Some of the managers of the Pennsylvania Hospital had been banished to Staunton, Virginia, for eight months in 1777. Dr. Phineas Bond had been put under house arrest as a Tory. At the Almshouse, Anthony Morris, Jr., a manager, had been killed in the war; some of the managers had been disqualified on account of Tory sympathies; one of the doctors of the Almshouse staff, Adam Kuhn, had been forced to flee the country, but had been readmitted. Poverty and distress were extreme. The Army and Navy were disbanding. Twenty-three sailors, ten of their wives and three of their children, all "strangers" (that is, not citizens of Philadelphia City) were in "the House," together with Continental Army men, British deserters, the riff raff of the city, and the unfortunates of the inflation for whom a special partition was being put up because "they were accustomed to better conditions." Dr. Morgan, after shameful treatment in the Army though vindicated later by Congress, had returned from the war to find his library sacked by the British, his College with its estates and franchises taken away by the

[21] *Loc. cit.* (footnote 19), p. 314.

legislature. Morgan never recovered from his misfortunes. In six years he was dead. Dr. Benjamin Rush says: [22]

> October 15, 1789. This afternoon I was called to visit Dr. Morgan, but found him dead in a small hovel, surrounded with books and papers, on a light dirty bed. . . . His disorder was Influenza, but he had been previously debilitated by many other disorders. What a change from his former rank and prospects in Life! The man who once filled half the world with his name, had now scarcely friends enough to bury him.

Norris, quoting Meigs in 1821, says: [23]

> Morgan is forgotten in Philadelphia. Memory of him has in one third of a century gone so completely, that of the many hundreds who partake of the benefits of this school, very few have ever heard his name.

With Morgan's death in 1789 and Franklin's in 1790 the formative period we have been discussing was over. The custom of treating the poor without a fee for the doctor introduced into Philadelphia by Franklin and the three physicians, gradually became general throughout the United States. Though the medical staff of the Philadelphia Almshouse continued to be paid a salary after it was called a hospital, the amount remained the same and its value gradually grew smaller. In 1850 it was $100 a year. Eventually the staff began to serve without a fee, probably during the reorganization which began in 1854.

In the fifty-two voluntary hospitals that have sprung up in Philadelphia City since the Almshouse was established, it is also the almost universal custom for the clinical medical staffs to serve ward patients with no direct monetary fee. It is only the private beds of these hospitals which provide opportunity to the doctors to charge fees of the patients. The doctors appointed to most of these hospitals conduct private offices outside and, in some cases within the hospital. The charge for ward beds is about eighty dollars a week. The patients cannot understand and, indeed, are loath to believe, that the medical service is being provided without charge. Dr. John Morgan's thought that the patients should understand exactly how much they are paying for "Medicines" and how much for "Consultation" has not yet been carried into effect.

The system of outdoor relief of the sick poor of Philadelphia has been

[22] George W. Corner, *The Autobiography of Benjamin Rush, His "Travels Through Life" together with his Commonplace Book for 1789-1813.* Princeton University Press, 1948, p. 180.

[23] George W. Norris, *The Early History of Medicine in Philadelphia.* Philadelphia, 1886, p. 90.

inherited directly from the Overseers of the Poor who were appointed following the dissolution of the monasteries. In Philadelphia the overseers paid doctors for care in the home until the city county amalgamation in 1856, after which city poor doctors were paid, to treat the indigent at home. There was a short interruption of this service, when the second almshouse began in 1766, at which time all outdoor relief was stopped as an economy measure. Free treatment of the sick poor by the family physician has always been a matter of his personal choice. It is often a considerable part of a doctor's work.

In trying to determine why the medical profession has continued to serve the poor without any charge, many explanations have been offered. Some of these are: "the doctor gets a sense of satisfaction in such work"; "he is glad to do so for the experience"; or "on account of a feeling of public service"; for "an opportunity for research"; "in order to make professional contacts"; because "when he has acquired a competence in private practice he does not care about the lack of a fee"; or "there is a non monetary value in a hospital appointment." [24] The fact is that it is a tradition of our profession, springing from deep religious roots, from time immemorial. We at Blockley feel it has been a privilege to serve the poor. If we should give up our desire to look after the unfortunate "poor and miserable objects" mentioned by Franklin, the world would be taking a step backward. When we began in Philadelphia to have institutional care of the sick poor, we put a large letter P on the back of the pauper, who wore a special garb. But now our viewpoint has changed. We have vast national drives, into which the charitably inclined pour millions of dollars for any disease, malady, or social affliction that is skillfully presented to their attention. It is now hard to decide who is the pauper who should receive these benefactions, because we feel that we want the need for care to be the criterion for service rather than the financial status. These mass efforts of modern charity have made it seem unnecessary that doctors should continue to perform their services free on an ever increasing scale.

It may be that Benjamin Franklin left one answer to our problem when he advocated insurance which may eventully cover the medical cost of free care of the poor.

[24] Michael M. Davis and C. Rufus Rorem, *The Crisis in Hospital Finance and Other Studies in Hospital Economics*. The University of Chicago Press, 1932, p. 83 ff.

DEBORAH SAMPSON, ALIAS ROBERT SHIRTLIFF, FIGHTING FEMALE OF THE CONTINENTAL LINE *

WM. FREDERICK NORWOOD **

New England's sense of propriety was shocked when, in the fall of 1783, it became known that a 21-year-old Norfolk County girl, Deborah Sampson, had in 1782 disguised herself as a man and joined the Continental Army. To believe further that she served for seventeen months in the Fourth Massachusetts Regiment of Foot without the disclosure of her sex taxed the credulity of both men and women.

The position of New England women in the eighteenth century decreed that females, if they received any education, were to be instructed principally in domestic duties and religion. Daughters of wealthy citizens, to be sure, received their upbringing and instruction within the context of refinement and culture. Less fortunate girls were at times assigned to outdoor manual duties and they had limited access to books and other literature. Although women had comparatively little freedom and independence compared with contemporary males, in the religious economy woman occupied a pedestal of dignity and respect. According to the teachings of the Mathers and Samuel Sewall the position of the wife and mother was of supreme importance in the growth of the community and the nation. Godly women respected their husbands, bore children, served the family, attended church, and in spare moments contemplated the state of the elect. An inviolate aura of sacredness surrounded the respectable woman regardless of her social rank.[1]

The unique conduct of Deborah Sampson raises several questions. What influenced her to break tradition? How did she conduct herself while in the service? What was her medical history while in service? What effect did her military service have on her after life? In general what sort of a character and personality was Deborah Sampson, alias Robert Shirtliff, who because of beardless healthy cheeks was nicknamed "our blooming soldier?"

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[1] Mary Sumner Benson, *Women in Eighteenth-Century America, A Study of Opinion and Social Usage*. New York: Columbia University Press, 1935, pp. 11-33, 314-16.

Deborah Sampson was born on December 17, 1760, in the village of Plympton, Massachusetts. In the ancestral line of her father, Jonathan Sampson, Jr., appear the distinguished names of Alden and Standish. Her mother, Deborah Bradford, was the daughter of Elisha Bradford and Bathsheba LeBroche, the former having descended from William Bradford (1588-1657). Reduced in status by his sudden and unfortunate loss of wealth Elisha Bradford left little or no bequest to Deborah and his other children.[2]

When Jonathan Sampson, Jr. married Deborah Bradford, he expected soon to inherit an estate from his father. Although the probate records seem to reflect a fair distribution of the estate in 1759, Jonathan always contended that his brother-in-law managed to deprive him of his rightful share.[3] Both Jonathan and Deborah had excuse to regard themselves as the children of misfortune. Despairing of his ability to accumulate an estate through ordinary domestic employment, Jonathan went to sea soon after the unsatisfactory settlement of his father's legacy and before the birth of his daughter Deborah. Whether he had good prospects in a seafaring career or was merely running away from frustration is not known. His return visits were reportedly few. Before long he was reported a casualty at sea. His widow undertook to support and keep together her five children but sickness and misfortune forced her to surrender them for placement in various homes.[4]

[2] [Herman Mann], *The Female Review: or Memoirs of an American Young Lady.* Dedham: Nathaniel and Benjamin Heaton, 1797, pp. 18-22.

[3] [Herman Mann], Introduction and Notes by John Adams Vinton, *The Female Review, the Life of Deborah Sampson.* Boston: J. K. Wiggin & Wm. Parsons Lunt, 1866, p. 50, f. n. 4.

[4] Both fact and fiction are incorporated in Herman Mann's Life of Deborah Sampson. The general outline of the story is without doubt correct but many details are doubtful and some are rather fantastic. Apparently both Mann and Deborah Sampson (Mrs. Gannett) were not satisfied with the 1797 edition of *The Female Review.* Eventually, with additional information in hand, a new manuscript was prepared in the fashion of a memoir. Mrs. Gannett was represented as telling her story but the text was still in the ponderous and overwrought language of Mann. It was agreed that the story would be published after Mrs. Gannett's death which occurred in 1827. A severe illness prevented Mann from completing and publishing the book before his death in 1833. His son, Herman Mann, Jr. undertook to complete the manuscript, exercising considerable liberty in a new draft which was finished in 1850. His death in 1851 probably disrupted publication plans. *The New England Historical and Genealogical Register,* XVIII, October 1864, pp. 348-86, under the title "Proceedings" of a July 6, 1864 meeting reports that Reverend John A. Vinton of South Boston read a very interesting and carefully prepared paper on Deborah Sampson. Some of the facts presented indicate that he had not completed his critical study as reflected in his analysis published in 1866. See f. n. 3 above.

Deborah was scarcely five years old when she was separated from her mother. An elderly kinswoman assumed responsibility for Deborah and her education. Death of this foster parent soon disrupted Deborah's security and caused her to conclude, as she later stated, that she "was born to be unfortunate." Deborah's mother then placed her with an elderly Mrs. Thacher of Middleboro. Because she was "of a slender constitution" and perhaps could not do the work expected of her, ten-year-old Deborah was indentured to Deacon Benjamin Thomas of Middleboro, who became her guardian until she was 18 years of age.[5]

Although he did not possess a cultivated mind in all respects, the deacon was well versed in the Scriptures and conscientiously performed his known duties. His substantial farmhouse built principally of white oak cloistered a household operated along the lines of Congregationalism and New England thrift. The deacon's sizable family—more boys than girls—lived well on a productive farm. Deborah's opportunity for an education in the district school was not equivalent to the privileges which the deacon conferred on his children.

In all his descriptions of Deborah the child and youth, her biographer, Herman Mann, strains for an effect. He also introduces extraneous matter and indulges in efforts to edify. Being the object of his literary devotions, Deborah is adorned with praise and virtue. It seems entirely safe, however, to conclude that she developed qualities of industry, self-reliance, and a more than ordinary thirst for knowledge. Mortified by a fear of illiteracy she learned to read the school books and copy material of the children around her. In time she found herself a fairly competent reader and master of a legible hand.[6]

Herman Mann pictures Deborah as having more than ordinary powers

This book reprinted the 1797 text but added criticisms and corrections in the form of copious footnotes, much of which is an abstracting or quoting of the so-called memoirs manuscript to which Vinton had free access. Vinton also had access to Reverend Stillman Pratt's account of Deborah Sampson which was apparently published in *The Middleborough Gazette*. Vinton's work was republished as *Extra Number 47 of the Magazine of History with Notes and Queries*, 1916, 166 pp.

The original edition by Mann, and Vinton's notes are the principal sources for this study. The memoirs manuscript is the property of the Dedham Massachusetts Historical Society.

[5] Mann in his *Female Review*, p. 26, introduces the deacon as Jeremiah Thomas. Vinton in his notes in the 1866 edition offers sufficient evidence that Deborah's guardian was Benjamin Thomas, chosen deacon of the First Church of Middleborough in 1776. In 1782 he became a member of the Massachusetts legislature and in 1788 was elected to the Federal Constitutional Convention. He died in 1800.

[6] [Herman Mann], *The Female Review*, pp. 26-40.

of intellectual perception and philosophical insight. Through her feeling of kinship with nature she developed a sense of belonging to the Deity and Creation. Her human sympathy aroused by the blockade of Boston in 1774, she gave of her limited funds for the relief of the suffering populace. She followed carefully the news of the day and was deeply moved when the fortunes of liberty struck a new and strident note at Lexington on April 19, 1775.

Four nights before the Lexington encounter fourteen-year-old Deborah had a frightening and ominous dream which her biographer recounts in great detail. She was viewing an inspiring pastoral scene in which all life was harmoniously in tune. Then suddenly nature was convulsed. Lightning, thunder, earthquake, and volcano threatened all creation. Deborah's reason seemed to forsake her as she saw a huge and hideous serpent uncoil from the sea and threaten the land with carnage and destruction. She fled but, on looking back, saw the monster in close pursuit, leaving the streets drenched in blood. Overwhelmed with fear she fell in a swoon. Next the dreamer found herself in her apartment where she hoped for security, but the serpent reappeared, more frightening in form and size. He moved to the bedside where the frantic child cowered neath his lowering head, with eyes of fire and a gaping mouth spiked with deadly teeth and a tongue tipped with a lethal sting. Powerless to scream, she could only cover her head and await destruction.

At length she heard a voice: "Arise, stand on your feet, gird yourself, and prepare to encounter your enemy." Obedient, she arose and the monster poised himself as if to swallow her whole before she could robe herself. In her extremity she called upon God and there appeared at her feet a bludgeon. Strengthened for combat she seized the weapon and wielded it mightily. She drove the creature from her room and at length broke him into pieces, but the pieces reformed themselves into an ox which attempted to gore her. The gallant heroine renewed her attack and beat the beast into segments which had the constituency of jelly. She immediately awoke.[7] Feeling that the dream presaged some great event, she recorded it in her diary but made no effort to interpret it or to discuss it with others.

[7] *Ibid.*, pp. 62-68. Vinton, *op. cit.*, p. 79, f. n. 16, says that the memoir manuscript represents Deborah as having had the dream on three successive nights, the last being the night of the Lexington alarm. The memoir recounts a much longer dream in high-flown language and is represented as a prophecy of the American Revolution. Although Vinton thinks the dream originated in Mann's imaginative brain, it would not have been strange for a fourteen-year-old girl to have a dream of fantastic horror.

How much actual time Deborah Sampson was permitted to attend school is not known but there is little doubt that she did acquire some intellectual habits and cultural aplomb for we find her teaching school for a term of six months before her eighteenth birthday, when her legal ties to Deacon Thomas terminated. Teaching a second term suggests that she achieved some success as an elementary educator, but she was convinced that her education rather than her native talent was inadequate for the task.[8]

An uncommon emotional agitation manifested itself throughout New England in 1780. The stir was not confined to any age group. Some observers saw in these unusual manifestations the working of the Holy Spirit. Herman Mann has his heroine examining the situation coolly. She was not sure whether it was a war mischief taken advantage of by the clergy to add to their church rolls, a voluntary act of the mind, a pandemic cachexia, or a true manifestation of the "Divine Character." She concluded, according to Mann, to have no part of it, reasoning that the victim when in such a delirium lost his powers of reason and was disqualified for useful business. Further she felt that the active participants in the movement were incapacitated for the adoration and worship of Deity in a sound and dignified manner.[9]

Actually Deborah was moved by the challenge of the hour. A Baptist divine, Asa Hunt, was holding forth in a locality known as "The Rock." Deborah was induced to attend his meetings. The records of the First Baptist Church of Middleboro show that she was received into membership on November 12, 1780. The Congregationalist minister, Abraham Camp, at the First Church, where she had attended with the Thomases, held Deborah in high regard and regretted her departure.[10]

When the religious fervor of the time had spent itself, Deborah seemed to lose the feelings of security and certitude which she had momentarily associated with the revival. She began to speak lightly of experimental religion. Her freedom from indenture, her teaching experience, her love

[8] Mann, *op. cit.*, pp. 92-93. This school term was probably a summer session in 1779. The range of subjects was no doubt limited. Such books as *The New England Primer*, *The Psalter*, a few *Testaments*, and a speller were available. Deborah brought some of her few books to school. Boys emphasized penmanship while girls practiced knitting and sewing. One lad brought a copy of Fisher's *Young Man's Best Companion*.

[9] Mann, *op. cit.*, pp. 92-100. Mann takes care to assure his readers that Deborah was possessed of deep religious qualities and would not have questioned the validity of the Scriptures.

[10] Vinton, *op. cit.*, p. 102, f. n. *. Abraham Camp was only temporarily filling the pulpit at Middleborough at the time of Deborah's change.

of nature, and her interest in national and world events were pushing back the borders of her universe. She longed to travel, but how could a young single woman travel and be within the bounds of propriety? "If only I were a man!" Evil thought—feeling condemned, she suppressed it only to have it return. For months the dilemma continued—weighing the conventional role of woman against her ambitions to see places and people. She determined to break the bonds which held her. She would travel as a gentleman.

Securing a handsome suit of men's apparel, she secretly donned it and went boldly to a fortune-teller. Not discerning her disguise he received her as a blithe young man and spoke flatteringly of "his" propensities for uncommon enterprises. Thus fortified, she tried her male attire on other occasions. Even her mother, whom she visited in Plympton, did not recognize her in the costume. The prohibitive cost of travel forced her to reconsider.[11] A bit of teaching, feeding pigs and poultry, and spinning and looming gave no promise of sufficient funds to finance travel. It required but little further rationalizing to adopt the scheme of joining the Continental Army. Her desire for travel would in all probability be satisfied, her travel expense would be covered, and she would accumulate money for herself. Although Cornwallis had already surrendered at Yorktown, the Army still needed men and the patriotic motive was appealing.

Deborah's mother had long doted on her felicity with a young gentleman of fortune and agreeable deportment. Mann naively, if not fallaciously, concluded that she would have married him had not her love of country taken precedence. Long afterward in her memoirs manuscript she made plain that she was never in love with "this lump of a man" whom she finally repulsed when he made a fool of himself while intoxicated.

Displeased with her venture in romance, unsatisfied with her status in society, and conscious of her educational limitations, Deborah sought an escape. Her father, for similar reasons, had joined the merchant marines. Deborah chose the Army. Reverend John Adams Vinton in his 1866 notes on Deborah Sampson adds to the 1797 account a version of her

[11] *Ibid.*, p. 113, f. n. 27. There are conflicting tales about how Deborah secured her male attire. Vinton claims that she spun and wove the material with her own hands and employed a tailor to make clothing. He quotes Reverend S. Pratt's version that after winter weeks of spinning and weaving she retired on warm spring days to a secluded spot in the Borden Hills and there cut and made a coat, vest, and breeches. Evidence indicates that Deborah's first wearing of male attire was no sooner than the fall of 1781.

recruiting experience which is plausible as well as gossipy but spoils Herman Mann's idyllic tale. It might be called a dress rehearsal.

Miss Sampson was staying in the home of Captain Benjamin Leonard. Jennie, a negro family servant, secretly assisted Deborah in dressing herself in the clothes of the captain's son. Thus clad she went to a recruiting office at the home of Mr. Israel Wood and enlisted under the name of Timothy Thayer and received the authorized bounty paid to volunteers. At a tavern she became intoxicated and conducted herself in an indecent manner. Returning home, she crept to bed with Jennie and in the morning resumed her female attire and employment as if nothing had happened. The enlistment seems to have been half frolic and half experiment.

When the Middleboro enlistees were called up, "Timothy Thayer" could not be located. It happened that an old lady who had watched "him" enlist noted that "he" held the pen just like Deborah Sampson who, because of a felon, had lost the free use of her forefinger. Deborah was exposed and returned the unspent portion of the bounty and agreed to stay out of sight, lest punishment overtake her.[12] This playful preamble took place several months before her final enlistment and may have occurred prior to her visit to the fortune-teller. She had no intention of keeping Samuel Leonard's clothes and there can be no doubt that she ultimately provided male clothing for herself in a legitimate fashion.

The "Timothy Thayer" episode strengthened her determination to join the Army, which she did in May, 1782. Her final mental struggle was apparently not over the moral or social impolicy of the act but over the question of whether she should take anyone into her confidence, particularly her mother or the Thomases. The very nature of her adventure, she concluded, precluded the expediency of telling any person.

Clandestine preparations were made for a quiet midnight departure. After several days on foot she came to Bedford, a seaport, where the captain of an American privateer signed "him" as a waiter. Hearing that the captain was a hard master at sea, she made pretext for returning her month's advance and quietly went to Boston.[13] Then heading westward she came to Bellingham where she fell in with a "speculator," the name applied to those who in certain communities promoted recruitment

[12] *Ibid.*, p. 124, f. n. 34. Vinton goes to considerable length to authenticate this first enlistment story. Another author confuses it with her second or final enlistment. See G. M. Fessenden, "A Genealogy of the Bradford Family," *The New England Historical and Genealogical Register*, IV (January, 1850, pp. 39-50), p. 48.

[13] Mann, *op. cit.*, p. 126, f. n. Apparently Mann had heard rumors of the "Timothy Thayer" episode and attributed it to the Bedford incident.

by enlisting men who would enroll in the place of young men subject to call but who preferred to pay their way out. The volunteer received a sum of money after the speculator took his cut.[14] Deborah's induction was at Worcester. She joined the Army on May 20, 1782.[15]

Miss Sampson's memoirs manuscript tells of a highly fatiguing ten to twelve days' march to West Point, New York. At the close of a cold and rainy day Deborah fainted in front of a tavern fireplace. The innkeeper's attractive wife revived "him" with cordials and refreshments and insisted that the frail young recruit take her place in a warm bed with the innkeeper.

Once in West Point she found herself in the Fourth Massachusetts Regiment, commanded by Colonel William Shepard of Westfield but soon succeeded by Colonel Henry Jackson of Boston. Her captain was George Webb and the brigade commander was Brigadier General John Paterson.

On the second day she was issued a French fusee, a knapsack, cartridge box, and 30 cartridges. Her civilian attire was discarded for the handsome blue and white uniform then issued to new recruits. Mann described his heroine as larger than average—five feet seven inches in height. Her facial features were regular but not beautiful. She had penetrating eyes and a clear complexion. But her aspect, he added, is more "masculine and serene" than "effeminate and sillily jocose." He conceded that her waist might displease a coquette but ladies of taste considered her limbs handsome when in masculine garb. She was erect, graceful, mild in gesture but quick and strong in action. Her voice was not disagreeable for a female. Herman Mann modestly buried in a footnote the pertinent

[14] The eligible male population of each town was divided into what was called classes, each of which was obliged to furnish a soldier. Some classes paid a sizable bounty for a volunteer. Deborah was accepted for a class in Uxbridge, Worcester County. The muster-master was Captain Eliphalet Thorp of Dedham. Deborah's enlistment as Robert Shirtliff is attested to by Captain Thorp in a document in the office of the Secretary of the Commonwealth of Massachusetts. See Vinton *op. cit.*, Introduction, pp. xxv, xxvi. A receipt, "Worcester, May 23, 1782, for bounty paid said Shurtlieff [note spelling] by Noah Taft, Chairman Class No. 2 of the town of Uxbridge, to serve in the Continental Army for the term of 3 years" is recorded in *Massachusetts Soldiers and Sailors of the Revolutionary War*. Boston, 1906, p. 185. There were several variations in the spelling of Shirtliff.

[15] Herman Mann has Miss Sampson joining the army in the spring of 1781 a full year earlier. Mrs. E. S. Ellet, "Heroic Women of the Revolution," *Godey's Lady's Book*, XXXVII (July, 1848), pp. 6-9; and Elizabeth F. Ellet, *The Women of the American Revolution*, 3 vols., New York, Charles Scribner, 1852, vol. II, pp. 127-30, have Deborah enlisting in 1778. Mrs. Ellet also has her spending seven weeks after enlistment in the home of Captain Nathan Thayer in Medway. A "love passage" between "Robert Shirtliff" and Mrs. Thayer's niece during this time seems entirely apocryphal.

observation that during her time of disguise she wore a bandage about her breasts for a very different purpose from that which led women to wear restricting garments about the waist. He noted also her "waist had every natural convenience for augmentation." [16] So this is Deborah Sampson, alias Robert Shirtliff, fighting female of the Continental line.

While professing to write an historical account, Mann indulges in the fiction of having Deborah enlist a year before she actually did. Perhaps this was to represent her as a seasoned infantryman before Cornwallis' defeat at Yorktown. Just before the final assault she is made a non-commissioned officer and fights her way with bayonet into a British held redoubt, disemboweling redcoats as she goes. Reverend Vinton noted that Deborah's manuscript memoirs of the Yorktown engagement as well as of other military activities in which Deborah obviously had no part, followed closely, even to style and vocabulary, the well-known account in Dr. James Thacher's *Military Journal* published in 1823.[17, 18]

Back in Middleboro and Plympton some people decided to do something about Deborah's casual disappearance. Mann pieces this segment of the story into the spring of 1782, which of course could not have been until later, and probably did occur during the summer of 1782. One of her brothers took off on a fruitless search to the east and north while her ardent lover chose to go westward. The unnamed suitor journeyed to West Point and scanned the many strange faces. Once, so the story goes, he was actually in a group where Deborah was present but probably did not see her. But Deborah saw him and hastened from sight to conceal "the big tear" which "trembled in her eye." Her emotional response was more at the thought of her mother's anxiety than of the jilted lover's persistence. After days of effort to hush the burden of her mind, without revealing her location or vocation, she wrote to her mother, in part:

> I am in a large but well regulated family. My employment is agreeable, although it is somewhat different and more intense than it was at home. . . . My superintendents are indulgent; but to a punctilio, they demand a due observance of decorum and propriety of conduct. . . . Be not too much troubled . . . as I will endeavor to make that prudence and virtue my model for which, I own, I am much indebted to those who took charge of my youth.[19]

[16] Mann, *op. cit.*, f. n. p. 133.

[17] James Thacher, *A Military Journal of the American Revolutionary War*, from 1775 to 1783. Boston: Richardson and Lord, 1823, pp. 332-50.

[18] Thacher, *loc. cit.* Mann tells about a mass inoculation of troops in the hospital, which Deborah avoided for fear of exposing her sex. He places the inoculation event in January 1782 before Deborah enlisted. He treated about 200, including women and children.

[19] Mann, *op. cit.*, pp. 167-68.

Still others were concerned. In its minutes of September 3, 1782, the First Baptist Church of Middleboro mournfully recorded:

> The Church consider'd the case of Deborah Sampson, a member of this Church, who last Spring was accused of dressing in men's clothes, and enlisting in the Army, and although she was not convicted, yet was strongly suspected of being guilty, and for some time before behaved very loose and unchristianlike, and at last left our parts in a sudden manner, and it is not known among us where she is gone, and after considerable discourse, it appeared that as several brethren had labour'd with her before she went away, without obtaining satisfaction, concluded it is the Church's duty to withdraw fellowship until she returns and makes Christian satisfaction.[20]

Deborah's first actual combat came during the summer of 1782. Scattered units of British troops and Tory bandits or refugees were still active in New York and New Jersey . On a scouting expedition near Tappan Bay, Deborah received a superficial head wound and a much more severe wound in the right thigh just below the groin. Taken to a French Army hospital nearby, she permitted her head to be dressed but kept secret the thigh wound. In a moment of privacy she cleansed the wound with rum and with a penknife and needle extracted the bullet.[21]

Knowing that she could not return to immediate active duty with her company she got permission to nurse a critically ill soldier who could not be moved. They were housed with an old Dutchman, named Van Tassel, who turned out to be pro-Tory, but whose young daughter was more than ordinarily sympathetic. Her patient died and she underwent harrowing experiences before returning safely to her company.

The fall and winter of 1782-83 were packed with scouting, skirmishing, a "boisterous cruise down the Hudson to Albany," and a forced march with volunteers to aid General Philip Schuyler against the frontier Indians. She saw Indian barbarity at its worst and according to Mann returned to headquarters, her half-clad feet making crimson the snow.

On the first of April, General Paterson rewarded Robert Shirtliff by making "him" his waiter. Knowing she had the General's commendation and approval, Deborah spared no effort to please him. The General thought that such courage and refinement could only exist in the superior order of his sex. Mann felt compelled to moralize thus:

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[20] Vinton, *op. cit.*, p. xxviii quotes the church action. The date, September 3, 1782, confirms the spring of 1782 as the time of Deborah's final enlistment. It does not appear that Deborah was ever restored to fellowship in the Baptist or any other church.

[21] Vinton, *op. cit.*, p. 171, f. n. 71. The wound never ceased to be bothersome.

. . . Females, whilst you see the avidity of a maiden in her teens confronting dangers and made a veteran example in war, you need only half the assiduity in your proper, domestic sphere, to render your charms completely irresistible.

An unlikely tale, omitted from the memoirs, bears upon the general's summer-time order that all soldiers take regular baths in the Hudson. Deborah found a lovely fountain that percolated from high rocks at the river bank, thickly enclosed with aspen and alder. Here, unnoticed, she carried out orders. There can be no doubt that she cleverly concealed her secret until some months later. Mann merely states that " she was expert in undressing with the rest."

During this summer (1783) a detachment of 1500 troops were ordered from West Point to Philadelphia to quell a mutiny of some Continental troops. Deborah was just beginning to enjoy the society of both sexes in Philadelphia when she was stricken with fever and taken to the hospital. She sunk into a coma from which Dr. Barnabas Binney, the surgeon, did not expect her to recover. Undertakers stood by, but Nurse Jones detected signs of life and called Dr. Binney. He thrust his hand under Robert Shirtliff's clothing to see if any heartbeat was perceptible. After removing a light inner vestment he discovered both the fact and the facts of life. The latter he revealed only to Mrs. Parker the matron, who removed Miss Sampson to her own apartment where she eventually recovered.

Among Robert Shirtliff's new acquaintances was an attractive young lady of fortune from Baltimore. On " his " third day in the hospital she brought to " him " a napkin of choice fresh fruits and an unsigned letter expressing deep sympathy and affection. Like favors continued during Deborah's illness but " she knew not in whose bosom the passion vibrated."

Fearing that the surgeon had made revealing discoveries during her unconscious state, Deborah became subject to repeated periods of depression. When the doctor asked her if she had any confidant in the army she said, no, but was so visibly disturbed that he waived further discussion. She responded to his suggestion that she accompany Col. Benjamin Tupper of Massachusetts who, with two Philadelphia gentlemen, was soon to leave on a business and speculative tour into the Ohio country.

The party made its first stopover in Baltimore. Following the lead of an anonymous note of invitation, Deborah at length found herself alone in an elegant parlor with the admiring nymph of Baltimore, who confessed she had sent the fruit and letters of affection. Mann describes two days of prudent and dignified companionship that must have " fired the breast of an anchorite." Promising to visit the trusting young heiress on

her return, Deborah left for Ohio. Somewhere in the mountain area she suffered a relapse of her fever and was left by her party with the Indians. She recovered and, amidst a chain of startling experiences, saved the life of a white slave girl held by the Indians, by purchasing her for "his" wife.[22]

Returning to Baltimore, Deborah assisted in getting the young bride on her way to relatives and then concentrated on the problem of dissolving the flattering but impractical ties which bound her to the Baltimore debutante. Deborah pleaded poverty and lack of both social status and education. The determined teenage lover offered to educate her soldier boy and to wait until he achieved her social level. Deborah longed to undeceive the sentimental child but was at a loss to devise a plan. She made some half promises to return in the spring and departed, taking with her a love gift of six Holland shirts, 25 guineas, and an elegant silver watch. Mann declared this affair to be a "remarkable instance of the origin of that species of love which renders the enjoyment of life satisfactory, and consumates the bliss of immortality . . . when managed with prudence."

Revived in spirits Deborah reached Philadelphia in early November, but the sight of Dr. Binney aroused feelings of anxiety. On the day of her departure Dr. Binney handed her a letter addressed to General Paterson who with his troops had returned to West Point. In route she suffered shipwreck in New York harbor, losing her trunk, military journal, and money, but saving her watch and the letter for General Paterson.

In West Point the General was happy to receive her, having long concluded that she lay in Philadelphia's potter's field. Deborah quiveringly handed him the letter and hastily withdrew. In an hour she was back on orders. With little preamble the general came straight to the point: "Does that martial attire, which now glitters on your body, conceal a female's form?" Deborah sunk to the floor in a faint. When revived she banished all subterfuge and said, "I am a female." Exclaimed the general, "Can it be so?"

Soon provided with woman's attire and a half hour for transformation she came forth exhibiting for the general some feminine graces. "This is truly theatrical," said General Paterson, "I will summon Jackson, and see if he knows you." Colonel Jackson was introduced to Miss Deborah Sampson with whom he carried on a polite conversation until the general

[22] The ramble toward Ohio probably took place but the liberation of the white girl is more likely Mann's fabrication.

forced the fact upon him. Braced by her general and colonel and clad as a woman, Deborah was escorted through the camp, unrecognized by any of her fellows. As proof of her virtue, she requested that her mess mates be strictly interviewed. A veritable panic of surprise swept the camp. Groups sought an opportunity to talk with their fellow footman but when she was hidden from public gaze, surprise and curiosity shaded into unbelief.[23] On October 25, 1783, General Henry Knox issued an honorable discharge to Robert Shirtliff. Robert Shirtliff was no more.[24]

Deborah Sampson returned to Massachusetts but she avoided Middleboro, not knowing how she would be received. Continuing to wear her regimentals she assumed the name of her younger brother Ephraim Sampson and went to the farm of her uncle, a Mr. Waters of Stoughton. She spent the winter doing farm work and flirting with the girls of the neighborhood, who were much attracted by the handsome Continental veteran. It is incredible, as Mann assumed in 1797, that her uncle was not aware of her true identity.

A local observer was young Mr. Benjamin Gannett of Sharon, who was said to be skeptical of the gay newcomer and determined to learn if "he" was actually a man. With the coming of spring Deborah cast aside her disguise and became a woman in looks and employment. Cupid worked fast; Deborah Sampson and Benjamin Gannett were married April 7, 1784. She died in her sixty-seventh year, on April 29, 1827. Benjamin survived her for ten years. In general, their life together was normal for the times, but bordered on poverty. Deborah was subject to a frailness of health which was attributed to her wartime thigh injury from which she never fully recovered. She was sufficiently strong physically to bear three children, two girls and one boy.

In January 1792, on the basis of her military service, Deborah Gannett petitioned the Massachusetts Commonwealth for compensation for ser-

[23] William C. Nell (Introduction by Harriet Beecher Stowe), *The Colored Patriots of the American Revolution*. Boston: Robert F. Walcott, 1855, p. 23. Lemuel Burr, a negro veteran of Boston, is cited by Nell for his reminiscences of Deborah Sampson Gannett. Deborah's memoirs manuscript (Vinton, *op. cit.*, p. 222, f. n. 124) states that she was provided with an apartment and both female clothing and regimentals, but that she preferred the latter. This account ends with a morbid observation: "Many of the soldiers, and many of my own sex, were desirous to satisfy themselves as to the truth of what they heard; but, of course, it was impossible to gratify their curiosity."

[24] Mrs. Elizabeth F. Ellet tells an unsupportable tale of Dr. Binney's sending Deborah with a letter to General Washington who said not a word and issued a discharge. Nor is it true that she was invited to visit the seat of national government during Washington's administration and that while there Congress voted her a pension. See Ellet, *op. cit.*, pp. 134-35.

vices rendered. Appropriate supporting documents were submitted. She received approximately 100 dollars as an invalid pensioner. Mrs. Gannett subsequently applied to the Federal Government for a pension allowable to invalid veterans. She was allowed 48 dollars per annum, beginning January, 1803, which sum was subsequently increased several times. After Deborah's death, Benjamin appealed for aid on the basis of old age, poverty, and being the spouse of a deceased veteran. It took a special act of Congress (July 7, 1838) to authorize pension at the rate of 80 dollars per annum from March, 1831, but Benjamin died eighteen months before the action. His heirs and a lawyer benefitted.[25]

As a married woman in her community Mrs. Gannett exercised traits of courage, adventurousness, and perseverance and for some years chose to give no emphasis to her military experience. The extent of criticism she may have endured is not known. Without doubt she was an object of curiosity and gossip.[26] Gradually she came to be regarded as a heroine but she refused to capitalize publicly on her story until 1797 when Herman Mann published his prudish account which was subscribed to by many reputable citizens before publication.

Any royalties which may have accrued to Mrs. Gannett from Herman Mann's fanciful account probably amounted to very little. Financial difficulties as well as a desire for publicity must have led Mrs. Gannett to take to the platform in 1802. As might be expected, her lecture, delivered first at the Federal Street Theater in Boston and subsequently elsewhere, was published and sold by Herman Mann. Unfortunately he and not Mrs. Gannett wrote the address and she recited it from memory. A few lines of her personal diary at the time indicate that she traveled alone from town to town, handling her own bookings and advertising. The lecture itself seems to be a public acknowledgment of her "foible, an error of presumption." Seeking the indulgence of her critics, she offered no reason for her military interlude other than a genuine love of liberty and her country. Her address ended with an appeal to all to excel in the areas designed for them by nature. She added spirit to her prosaic lecture by demonstrating the manual of arms while attired in her regimentals. Wit-

[25] Vinton, *op. cit.*, in his introduction, pp. xv-xxvi, cites many documents in support of the pension aspects of the story. The present author has photostats of many of the originals. As late as 1916 the Teasury Department certified that under Act July 7, 1838, payment of $466.66 was made to William Ellis, attorney for Earl B. Gannett, the son, and Mary Gilbert, and Patience Gay, the married daughters.

[26] Mann's entire treatment of Deborah Sampson is defensive and apologetic. He seemed determined to give her a social, moral, and religious frame of reference.

nesses recalled she could "almost make the gun talk." Her diary reveals the opinion that she was well received, especially by the women. She may have been the first American woman to give public lectures for pay.[27]

So this is the life of Deborah Sampson, heroine of the Revolutionary War. There were other heroines—Margaret Cobrin and Mary Ludwig Hayes (Molly Pitcher) who rendered remarkable service to the Continental line, for which they both received Pennsylvania State pensions. But only Deborah Sampson successfully disguised herself as a man, enlisted, fought, and lived as a soldier for many months. That she came to her decision and carried it out in spite of strong contrary indications make her story medically and psychiatrically interesting.

[27] Mrs. Deborah Gannett, *An Address Delivered* with *Applause*. Dedham: H. Mann, 1802 (*Extra Number* No. 124, Vol. 31, 4, pp. 1-29; Reprinted from *Publications of the Sharon Historical Society*, No. 2, April, 1905, 15 pp.). Note the "Introduction" to the *Extra*, pp. 8-11.

THE FAMOUS HARRISON CASE AND ITS REPERCUSSIONS *

LINDEN F. EDWARDS

John Scott Harrison of North Bend, Ohio, was the fifth of ten children of President William Henry and Anna Symmes Harrison. He was the father of Benjamin Harrison, twenty-third President of the United States. His grandfather, also named Benjamin Harrison, of Berkeley Plantation, Charles City County, Virginia, was a distinguished political figure in the Old Dominion, having represented his county in the House of Burgesses; he was a member of the first Continental Congress, a signer of the Declaration of Independence and Governor of Virginia. John Scott Harrison, although devoting most of his life to managing both his and his father's farms, represented his district in the 33rd and 34th Congresses of the United States (1). An interesting historical side-light that should be mentioned is that he was named after Dr. John Scott of Lexington, Kentucky, who was the attending physician at his birth; like his father, William Henry Harrison, he too studied medicine but abandoned the profession. Dr. Charles Wilkins Short, the noted botanist of Kentucky, was his cousin and another cousin, Anna Short, was the wife of Dr. Benjamin Winslow Dudley, the renowned Kentucky lithotomist. One of his brothers, also named Benjamin, studied and practised medicine; his sister, Mary Symmes Harrison, married Dr. John Thornton; and his daughter, Betsey, married Dr. George S. Eaton (2). It is evident, therefore, that the Harrison family had a flair not only for politics but also for the profession of medicine.

On May 25, 1878, John Scott Harrison died suddenly of an unknown cause at the age of 74. Several circumstances prompted his three sons to take special precautions to protect his remains following interment. In the first place the popular notion prevailed that physicians would often disinter the body of a patient who died of an obscure disease in order to determine the cause of death; secondly, an alarming number of grave robberies had been reported in the press during the past few years; thirdly, Cincinnati, where several medical colleges were then located, was in close proximity; and fourthly, the most alarming factor was the discovery, a

* Read at the twenty-ninth annual meeting of the American Association of the History of Medicine, Durham, N. C., April 21, 1956.

few days prior to Mr. Harrison's funeral, of signs that a newly made grave in an adjoining lot in the cemetery had been disturbed. Consequently, after Mr. Harrison's grave was excavated a cemented brick vault was built into it, and after the casket was lowered into the vault it was roofed with a large flat stone, after which the grave was filled with stones, logs, and loose dirt. To add to the security, as was common custom at the time for those who could afford it, a watchman was engaged to visit the grave hourly every night for a week, and thereafter to pay occasional visits.

The newly made grave in the lot adjoining that where Mr. Harrison's remains were deposited in Congress Green Cemetery in North Bend was the burial site of a young friend of the Harrison family, by the name of Augustus Devin, who had died of tuberculosis a week or so prior to the death of Mr. Harrison. In the afternoon following Mr. Harrison's funeral, which was held at eleven a. m., May 29, 1878, his son, John, and his grandson, George Eaton, Jr., returned to the cemetery to investigate the grave of young Devin and discovered that his body was missing. Immediately suspicion was cast upon the medical colleges in Cincinnati, which they decided to visit and search. Accordingly, that evening they boarded a train for Cincinnati with this avowed purpose in mind. By the time search warrants were secured the hour had grown late so they decided to postpone their grim task until the following morning. The next morning armed with search warrants and accompanied by two civil officers, they went first to the Medical College of Ohio where they gained ready admittance. Failing to find the object of their search they were about ready to leave the building when one of the officers happened to see a rope hanging from a windlass through a closed trap-door in the floor. Lifting the trap-door and grasping the rope which hung taut, as though a weight were at the other end, he took hold of the crank of the windlass and after some winding brought to the surface a nude body, the head of which was covered with a cloth. Since the body showed no signs of emaciation and since the hair showing at the free edge of the cloth was gray, indicating the body to be that of a person much heavier and older than Augustus Devin, the two young men from North Bend informed the officers of their observation and, apparently disliking the ordeal, suggested that the body be let down into the dark chute. However, the officers' curiosity had been aroused and, against the protests of the young men, they removed the cloth. Thereupon, John Harrison and George Eaton became speechless with horror because, in spite of the absence of his long white beard which had been ruthlessly cut off, they recognized the body hanging before them

to be none other than John Scott Harrison. Upon recovering their speech they informed the officers of the identity of the body. They then debated as to whether they should engage an undertaker to return it to its place of sepulcher without apprising the rest of the family of their action in order to spare them the suffering and humiliation they themselves were experiencing.

Meanwhile the discovery had been made in North Bend that the body of Mr. Harrison was missing from its supposedly secure grave. Messages to that effect were dispatched to John Harrison in Cincinnati and to his brother Benjamin, whose home was in Indianapolis, Indiana. Benjamin boarded the first train available for Cincinnati, while at the same time another brother, Carter Harrison of North Bend, was bound for the same destination, both having in mind the same purpose, that is, of conducting a search of the medical colleges for the missing body of their father, not knowing until their arrival in Cincinnati that it had already been discovered under such peculiar circumstances (3). The first act of the three sons was to have the body of their father removed from the medical college to Spring Grove Cemetery in Cincinnati where it was temporarily deposited in a vault for security until some future date when it would be returned to North Bend (4).

The second act of Mr. Harrison's sons was to swear out a warrant for the arrest of the janitor of the Medical College of Ohio. The warrant was duly served and the janitor lodged in jail pending trial (3). The legal basis for this procedure was a provision in the Ohio law to the effect that any person found guilty of concealing a body illegally exhumed, knowing it to be such, was subject to a fine of one thousand dollars or six months imprisonment, or both, at the discretion of the court (5).

Meanwhile several arrests were made in North Bend, the parties apprehended being suspected of "stealing" Mr. Harrison's body. Also the man employed as grave watcher was naturally under suspicion, accused of either having slept through the night of May 29th or of having been bribed by the resurrectionist. He confessed that he had visited the grave only four times during the night, once at eleven, again at midnight, then at two a. m. and finally about daybreak (4).

Newspapers throughout the country carried the sensational story of the Harrison resurrection which startled the nation. Even the *New York Times* featured it on its front page (6). For several hours after the news release crowds of curious spectators thronged outside the medical college building where Mr. Harrison's body had been discovered (4). Consternation reigned among his friends and neighbors at North Bend and in

Indianapolis, where Benjamin Harrison resided. Proffers of assistance in detecting and bringing to punishment the perpetrators of the outrage were made by prominent members of the bar and other citizens (7).

The next morning following his arrest, the janitor of the medical college was released on bail of $5000.00 furnished by Dr. James T. Whittaker of the medical faculty (4). At his trial held on June 6, 1878, his attorneys waived examination and his case was consigned to the Grand Jury (8).

Members of the Harrison family as well as the press insisted that the professors of the medical college should disclose the identity of the person or persons who had violated Mr. Harrison's grave. In an attempt to defend the position of the faculty, Dr. Roberts Bartholow, Dean of the College, published an open letter in the Cincinnati papers in which he expressed the regrets of the faculty for the outrage and explained that under the existing circumstances bodies necessary for anatomical instruction had to be stolen, but that contrary to popular opinion, the men engaged in procuring subjects were unknown to the faculty. They delivered bodies to the college, received their stipulated fee and disappeared as mysteriously as they came (4).

In their testimonies before the Grand Jury in deliberations on the Harrison case, members of the faculty further elaborated upon their role in the procurement of bodies. It was pointed out that all transactions with procurers were handled by an intermediary person, that they were instructed to obtain bodies only from paupers' graves, that they were supposed to remove all marks of identification from the bodies and, when delivered, to place them in receptacles provided for their storage. In the case of Mr. Harrison's body it was denied that any member of the faculty, or the janitor, had any part in or knowledge of its resurrection until the morning of its discovery in the building, and the charge of concealment was emphatically denied (9).

In view of the state of the public mind, which seethed with indignation over the desecration of Mr. Harrison's grave, it must be conceded that anyone who would openly defend the medical faculty was forthrightly brave and bold. Such, however, was the case of the editor of a Cincinnati newpaper who courageously commented: "One lesson taught by the bitter experience of this afflicted household should be remembered when the Legislature meets. The law should be changed, not so as to take away the penalties against grave robbing, but changed so as to make the offense virtually impossible. Every thinking person will admit that medical colleges must have material for dissection. . . . So long as this necessity for subjects exists and there is no legal mode of obtaining the supply, the

business of resurrectionists must continue. The responsibility for this outrage rests ultimately with the Legislature (13)."

In the meantime the search for the missing body of Augustus Devin continued vigorously in all of the medical colleges in the city, but without success. Suddenly, a bomb exploded in the midst of the investigation: the janitor of the Miami Medical College disclosed that he had been in collusion with a notorious resurrectionist by the name of Dr. Morton, alias Gabriel, alias Dr. Christian, who had secured the janitor's permission to secrete in the college morgue stolen bodies preparatory to their shipment to other cities. When the news broke concerning the Harrison case and the search for Devin's body continued, Morton became alarmed and after hurriedly burying the only remaining body under the floor of the morgue made his escape from the city. As public indignation became more and more aroused over the Harrison case, the janitor became frightened about his illicit business arrangement with Morton and made a confession to Dr. William Clendennin of the faculty. The latter, in turn, imparted the stunning disclosure to the other members of the faculty after which the news was released to the press and civil authorities. According to the janitor's statement the shipment of bodies had been going on for about a month; following their preparation by injection they were packed in barrels and shipped to a fictitious address, namely, Quimby and Co., Ann Arbor, Mich. Quite obviously the contents of the barrels, mislabelled "pickles," were destined for the medical college in that city (10).

Immediately upon the disclosure of this astounding information two Cincinnati police officers boarded a train for Ann Arbor. There they readily traced the shipment of the "pickle-barrels" to the medical college where they found several bodies floating in brine in the storage vats of the morgue. Among these they thought they recognized the body of Augustus Devin, according to the description with which they had been furnished. However, in order to make certain they dispatched a message to North Bend requesting the presence of some member of the family to make positive identification. Accordingly George Eaton and Bernard Devin, brother of the deceased, journeyed to Ann Arbor where they corroborated the identification. They were then faced with another problem because difficulty was experienced in obtaining a release of young Devin's body from the medical college due to a demand made by the Demonstrator of Anatomy, Dr. William J. Herdman, that he be reimbursed the sum of $30.00 which was the expense he claimed he had incurred in procuring the body. The payment was indignantly refused and the body was finally

delivered to them by order of President Angell, but not until resort to legal process of replevin was threatened (11).

Meanwhile, in Cincinnati the janitor of the Miami Medical College was subpoenaed to appear before the Grand Jury for questioning but was released without indictment, as was the janitor of the Medical College of Ohio. An indictment was returned against Morton, the notorious resurrectionist, who, however, was never apprehended (12).

Ostensibly the Fates were cruel from the moment that John Harrison, Jr., and his nephew, George Eaton, Jr., had the impulse to open the grave of Augustus Devin, thereby starting into motion a chain of events which, although grievous and humiliating to the afflicted families involved, nevertheless proved subsequently to illustrate the old adage that " it's an ill wind that blows nobody good." Or as someone once succinctly expressed it—scandal oftentimes has its uses.

For several years the grave robbery evil had plagued the citizens of several Ohio communities, primarily because the method adopted by the lawmakers for its solution was as obsolete as the empirical method of treating the symptoms of a disease without attempting to seek out and treat its cause. That is, the legislature, as was true in several other states, enacted retaliatory laws, under the foolish assumption that such measures were preventive, whereas the simple expedient of passing a law legalizing the supply of dissection material would have eliminated the necessity for medical colleges to resort to body snatching. Unfortunately, in many instances a flagrant case of body snatching seemed to be required before the passage of an antomy law was forthcoming. For example, it took a " Doctors' Riot " in New York City in 1788 before the state of New York enacted its anatomy law; it was not until after a resurrection riot in New Haven that Connecticut passed its law; and as is well known, the Warburton Act of 1832 in England resulted from the murderous deeds committed by the fiendish body snatchers, Burke and Hare (13).

As regards the effect of the Harrison case in Ohio, it has been aptly said by Victor Robinson: " It took the body-snatching of a descendant of a signer of the Declaration of Independence, a Congressman who was the son of the ninth President of the United States, and the father of the twenty-third President of the United States to induce Ohio to act " (14).

Incidentally it should be pointed out that this author, in commenting upon the Harrison case, erroneously stated that Harrison's body was discovered " in the Ohio State Medical College."

Unquestionably the Harrison case, by serving to arouse the public and

press, provided the original stimulus which finally impelled the complacent lawmakers to act. However, two facts should be pointed out in this connection: in the first place, a veritable epidemic of body snatching occurred in Ohio subsequent to the Harrison case, especially in the fall and winter of 1878 and of 1879 (15); in the second place, immediate action by the Ohio Legislature was not forthcoming.

The Ohio General Assembly, meeting in Columbus in the year 1878, had already adjourned prior to the occurrence of the Harrison case (16), and therefore, the first repercussions of the case that were reflected in the actions of that august body were not evident until the following year. The 63rd session of the General Assembly opened January 7, 1879, on which date several bills bearing upon human dissection and for the punishment of unlawfully opening graves were introduced. The committees to which these bills were referred recommended indefinite postponement, which was agreed to. During this session the General Assembly was flooded with petitions, signed by Ohio physicians and laymen, praying the legislature to enact an anatomy law.

Hope was revived when a bill, entitled "An act to encourage and regulate the study of anatomy," was introduced by Representative James A. Norton, M. D., of Seneca County. On May 6, 1879, after considerable debate in the House of Representatives, the Norton bill passed that body by a vote of 62 to 16. However, when it reached the Ohio Senate, in spite of the recommendation for its passage by the committee to which it had been referred, a motion to table the bill was passed (17).

It is highly problematical how many years this bitter contest in the Ohio legislative halls would have continued had it not been for the recommendations of a commission appointed by the governor to revise Ohio's statutes. Since the commission was authorized not only to delete redundant and obsolete enactments but also to rectify omissions and amend imperfections in the original acts, one of its recommended amendments to the existing laws was a section providing for the legal acquisition of unclaimed bodies "for the purpose of medical or surgical study or dissection" (18). The report of this commission was published in 1880, and on February 19, 1881, the recommended amendment, entitled "An Act to encourage the study of Anatomy," was approved by the Ohio General Assembly (19). It may be concluded, therefore, that notwithstanding the indubitable influence of the Harrison case on the Ohio legislators in finally approving a law eliminating the necessity of such outrages as grave robbery in the state, an interval of three years passed before that influence fructified.

It is realized that fanciful speculation should be avoided in historical

accounts. Nevertheless, due to the wide publicity given to the Harrison resurrection episode, one is tempted to attribute to it a motivating factor which prompted the invention in 1878 of two devices that were patented and manufactured for the purpose of protecting recently buried bodies from would-be grave robbers. One of these inventions was a coffin torpedo marketed by a Columbus, Ohio, firm. It was made of iron, about an inch in diameter and six inches in length. It contained a charge of explosive and a detonating mechanism set to go off when the casket was tampered with (20). The other invention was a "burial-safe" patented and manufactured by a Cincinnati man, who advertised that it was specifically designed to protect the body from ghouls and marauders. It consisted of an iron cage-like container for a casket that could be securely locked and placed in the bottom of a grave or in a vault (21).

Another interesting repercussion of the Harrison case was that, following its disclosure, a delegation from Lawrenceburg, Indiana, visited North Bend and sought to negotiate with the Harrison family for the removal of the body of President William Henry Harrison to the cemetery in Lawrenceburg, where a handsome monument was to be erected over his grave (22). At that time his remains reposed in an old, crumbling brick vault which extended two or three feet above the surface of the earth and was covered with a thick layer of weeds and grass. It lacked any inscription of the identity of its occupant. Needless to say, the proposition of the Lawrenceburg delegation was not accepted. The state of neglect continued until 1924, 83 years following the death of President William Henry Harrison, when the present imposing monument was finally erected to his memory (23).

The mystery of the disappearance of the body of John Scott Harrison from its supposedly secure grave and of its discovery in a chute in the Medical College of Ohio still awaits final solution. Since the faculty and janitor of the Ohio Medical College continued to deny stoutly any implication in the affair and the ill-concealed satisfaction of their predicament by the faculty of one of the rival medical colleges in the city was so evident, it was strongly suspected that the deed was perpetrated out of malice, and that the body was secreted in the building in order to put the institution in an embarrassing situation.

Juettner opined that the identity of the perpetrators of the deed would never be known, concluding that "resurrectionists never reveal their secrets and the story of John Scott Harrison's body will probably never be told until the day of the final resurrection" (24).

The latest chapter of the Harrison case was written in 1914 when Dr.

Pinkney French of Los Angeles, California, in a news release, partly cleared up the mystery by volunteering the information that John Scott Harrison's body was exhumed by medical students, whose identities, however, were not revealed (25).

As to the final resting place of the body of John Scott Harrison, accounts vary considerably. The claim is made that interment was in the tomb of his father in North Bend (1, 24), however, according to the Superintendent of Parks and Memorials of the Ohio Historical Society, which has custody of the Harrison monument at North Bend, there is no record of his body being entombed in the monument. It is also claimed that the body was returned to the grave in Congress Green Cemetery where "armed guards kept watch for several weeks" (26), and according to another account it "still lies in an unmarked tomb" (2). Information furnished the writer reveals that the remains still repose in Spring Grove Cemetery in Cincinnati (27).

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(27) Personal communication from Mr. Joseph Wurtzelbacher, caretaker of the Harrison Memorial. This claim is substantiated by Dr. David A. Tucker, Jr., who informed the writer recently that one of Mr. Harrison's grandsons, had imparted to him the same information.

CHARLES E. MORGAN'S *ELECTRO-PHYSIOLOGY AND THERAPEUTICS*: AN UNKNOWN ENGLISH VERSION OF Du BOIS-REYMOND'S *THIERISCHE ELEKTRICITÄT* *

PAUL F. CRANEFIELD **

The appearance of Emil du Bois-Reymond's *Untersuchungen über thierische Elektricität* [1] in 1848 was a decisive event in the development of a physical orientation in German physiology. The boldly mechanistic philosophy of its preface sounded the anti-vitalistic theme of revolt against *Naturphilosophie*, while the equally bold claim of the author to have awakened the hundred year old dream of physicists and physiologists by demonstrating the identity of the nerve impulse with electricity drew into electrophysiological research such brilliant men as Pflüger, Bernstein, and Hermann, the giants of the classic period of electrophysiology. On the whole this purely biophysical phase of German physiology had little influence on English or American physiologists, who in earlier years studied chiefly in Paris and who studied with Ludwig only later at a time when the initial biophysical fervor had subsided.[2]

It is of all the more interest that at least one American, Charles E. Morgan, reached the laboratory of du Bois-Reymond between the years of 1857 and 1864, published research on an electrophysiological problem in a German journal, and undertook to translate du Bois-Reymond's treatise. As seen below his goal changed somewhat, but the book which finally appeared may at least be called an English version of the *Thierische Elektricität*, with added material of more recent date. Even today this forgotten volume provides an excellent source of material on the revival in electrophysiology which occurred in the years 1840 to 1865, a revival which is of great interest since the electrophysiology of today derives from

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[1] Emil du Bois-Reymond, *Untersuchungen über thierische Elektricität*, Berlin, Reimer, 1848-1884.

[2] Paul F. Cranefield, "The Organic Physics of 1847 and the Biophysics of Today" to appear in the *Journal of the History of Medicine*.

it, not from the early and more famous period of Galvani, Aldini, Humboldt, and Ritter.

Morgan's book [3] appeared posthumously in 1868 with no indication that it was primarily a translation. It seems quite possible that this omission resulted from the death of the author and not from any attempt at plagiarism, especially since the author did not live to write a preface. No attempt is made to conceal the sources, on the contrary the book has a bibliographical richness very unusual for its time. The first 171 pages consist of a survey of the theories of electricity and magnetism then current and of a description of various specialized galvanometers and stimulators. Introductory material of this sort was more or less standard throughout the 19th century in works in English on electrophysiology and electrotherapeutics. A certain unevenness in style and continuity suggests that most of this material is derived from other texts and research papers, but I have not attempted to collate it. No doubt the sources are the same as those referred to in Morgan's footnotes; in any event the material in those pages is not primarily from du Bois-Reymond. The first chapter on electrophysiology deals with "deriving vessels," i. e. recording electrodes, and is taken from the *Thierische Elektricität* and from du Bois-Reymond's "Beschreibung" [4] of 1863, as indeed is some material scattered irregularly through the first 171 pages. Chapter 13, which deals with the definition and scope of electrophysiology and chapter 14, "History of Galvanism and the 'Frog Current'" are condensed translations from vol. I of du Bois-Reymond's book. An analysis of a few pages of chapter 14 helps in understanding the relationship between the two books. Page 202 of Morgan corresponds to page 31 of du Bois-Reymond; page 203 to page 41; pages 204-207 to pages 46-53; and page 208 to page 56. Since Morgan often translates verbatim (and not very elegantly) it will be seen that he proceeded not as much by condensation as selection. He also resorted to rearrangement, as on pages

[3] Charles E. Morgan, *Electro-Physiology and Therapeutics*, New York, William Wood and Co., 1868, xvi + 714 pp., 171 Figures. I have seen five copies of this book. Three, at Duke, the New York Academy of Medicine, and the Kings County Medical Society, bear the imprint "Wm. Wood & Co." on the spine. Two, which I own, bear the imprint "London Robert Hardwicke" on the spine but are internally identical with the above. One of the Hardwicke copies was given to the Forbes Library, Northampton, Mass. by Wm. Wood & Co. in 1898. The Duke copy is stamped "R. J. Nunn, M. D. 119 1/4 York St. Savannah Ga." Evidently at least one copy found its way into the hands of a physician.

[4] Emil du Bois-Reymond, "Beschreibung einiger Vorrichtungen and Versuchsweisen zu elektrophysiologischen Zwecken" *Abhandlungen d. K. Akad. d. Wissensch. zu Berlin*, Phys. Klasse, 1863, p. 75.

215-216 where, in relating the history of galvanism, Morgan suddenly says: "In the spring of 1841 Prof. Emil du Bois-Reymond first commenced the repetition of Matteucci's experiments, with a view to test their exactness, and, if possible extend the results still further. In November, 1842, he communicated to Prof. Poggendorff an abstract of these researches. . . ." This material, which appears in the middle of Morgan's version of du Bois-Reymond's chapter 14, is actually taken from the beginning of du Bois-Reymond's preface. The chapter ends with the statement: "it at once becomes evident to every impartial man, that the honor of discovering the important law of the muscular current belongs incontestably to Prof. E. Du Bois Reymond." While this is a very faithful representation of du Bois-Reymond's own opinion, it is not a translation.

Morgan's book deals with electrophysiology from page 171 to page 683, and to determine the exact relationship between Morgan's book and du Bois-Reymond's would be an enormous job. This results from the fact that, although the backbone of Morgan's book is obviously material translated from the *Thierische Elecktricität*, Morgan added extensive material which had appeared in various papers and monographs between 1849 and 1865 (although du Bois-Reymond published parts of his book in 1860 and 1884, the most important material, and that from which Morgan drew, appeared in 1848 and 1849). Morgan referred to, and probably quoted without acknowledgment from papers and monographs by Heidenhain, Ranke, Fick, Kühne, Bernstein, Brücke, Wundt, Schiff, Pflüger, Rosenthal, Meissner, Henle, Helmholtz, Aeby, Donders, v. Bezold, Holmgren, Kölliker, Krause, Deiters, Hoppe-Seyler, Bilharz, Remak, Robin, Hermann, Marey, Kronecker, Thiry, Wunderlich, Traube, Billroth, Bernard, Ludwig, Voit, Pettenkofer, Eckhard, and Funk, not to mention E. Zurhelle and Miss N. Suslowa. Considerable material on the myographion and the conduction velocity of the nerve impulse was taken from Helmholtz's papers on these subjects, and Morgan explicitly acknowledged that chapter 17, "Arrangement of the Heterogeneous Components of the Primitive Muscular Fasciculus" was taken from Fick's *Medizinische Physik*.

The very wide range of authors cited and the wide range of topics covered might suggest that it is basically incorrect to think of Morgan's book as a translation or version of du Bois-Reymond's. Certainly Morgan's chapters on conduction velocity and muscle heat were not taken from the *Thierische Elektricität*. On the other hand the more than 500 pages devoted to electrophysiology by Morgan were solidly based on du Bois-

Reymond and contain long passages, pages, and even chapters taken directly from du Bois-Reymond. It would be quite reasonable to think of Morgan's book either as a review of the subject up to 1865 or as a revision of the *Thierische Elektricität* with the addition of recent material. Considered as a review, Morgan's book was not improved upon until the appearance of Biedermann's *Elektrophysiologie* in 1895. Considered as a revision of du Bois-Reymond's treatise it was better than the original, which was intolerably long, and more up to date.

Although the word therapeutics appeared in much larger type upon the title page than did the word electrophysiology, it is not the least of the merits of Morgan's book that only pages 682-708 were devoted to electrotherapy. There was no shortage, in 1868, of bad and long books on invalid applications of electricity in the clinic. Out of the maze of such applications Morgan selected exactly those which we would consider valid today: the excitation of muscles and their motor nerves, as introduced by Duchenne, and galvano-causty. He devoted less than a page to galvano-puncture for aneurism and the electrolysis of urinary calculi and concluded with the remark: "Such are the definite scientific applications of Electricity to Medical purposes: of the many others, it need only be said that they are either based on incorrect theory or *diagnosis* of disease, or an imperfect or incorrect knowledge of Electro-physiology; although I do not deny that *future* researches may enable us to do more, far more, than has hitherto been done in this direction."

Before turning to the contemporary reception of Morgan's book, it is of some interest to look at the few facts available about Morgan himself. Charles Edward Morgan was born in New Orleans, February 5, 1832.[5] He was the son of George Morgan, a member of a New York banking family (not connected with J. P. Morgan) and Paulina Amelie Drouillard Morgan. His father died in 1850 and it appears that his mother came to New York at that time. The family lived at 3 Fifth Avenue for many years and later at 27 Fifth Avenue. Morgan took both his A. B. and M. D. degrees at Columbia, the latter immediately before leaving for Europe in 1857 where he is stated to have remained for seven years studying under "Bernard, Robin, Le Compte, Virchow, Du Bois,

[5] This date, which appears on Morgan's grave, and in the Vital Statistics records in New Orleans, contradicts the statement found in Morgan's death certificate and obituary that he was 33 when he died. Excluding the remote possibility of error which could have arisen if a child had died in infancy and another child been given the same name a year or two later, this is presumably the right date. I am indebted to Miss Mary Louise Marshall of Tulane for help in verifying this date.

Reymond, and Hoppeseiler "[6] (*sic*). The internal evidence in the statement does not rouse too much faith in its reliability, but at least it is reasonably safe to conclude that Morgan spent some time in Paris as well as in Berlin. The enormous amount of German literature cited in Morgan's book, the dedication of the book to du Bois-Reymond, and the nature of the book point to Morgan having spent the bulk of his European stay in Berlin. He was perhaps the first American physiologist to study in Berlin rather than in Paris and perhaps the only American physiologist to come under the sway of the physical school at the peak of its self-confident, mechanistic ambitions. According to the obituary of John Vincent Morgan, M. D., Charles Morgan's younger brother, the latter also was abroad during the same years as Charles Morgan and he also studied under "Du Bois Reymond, and Virchow in Berlin, and Claude Bernard in Paris."[7] Since John Morgan was only 16 in 1857 and since he did not receive his M. D. degree from Columbia until 1870, this statement may be in error.

On his return to the United States in 1864, Charles Morgan was consultant in Diseases of the Skin at the Northern Dispensary, which still stands and is only a few blocks from Morgan's home. The death certificate of Morgan, on file in the Department of Health of the City of New York, states that he died on August 4, 1867, aged 33 years, from hemorrhage of the bowels, having been ill for nine days under the care of Dr. J. O. Smith. His funeral was at St. Bartholomew's Church and he is buried in Greenwood Cemetery, Brooklyn.

The only personal information about Morgan is in the obituaries published in *The Medical Record*[8] and *The Medical Register*.[9] (The note on Morgan in the 1912 edition of Kelly's biographical dictionary[10] is taken from the *Medical Register*.) He is said to have been an excellent linguist as well as to have been skilled in physics and mathematics. He was short, slight of frame, with a somewhat nervous manner and a feeble tenor voice. He is said to have been an excellent microscopist and artist, and to have cut the wood blocks which illustrate his book. The minutes of the library committee of the New York Academy of Medicine show that in July 1892 Morgan's sister, Mrs. William Oothout, gave Morgan's

[6] *The Medical Record*, *2*: 311-12 (1867).
[7] *The Medical Register*, N. Y., N. J., and Conn. *18*: 238 (1880/81).
[8] *Op. cit.* (footnote 6).
[9] *The Medical Register of the City of New York*, *6*: 330-331 (1868/69).
[10] *Cyclopedia of American Medical Biography*, H. A. Kelly, Philadelphia, 1912, v. 2, p. 187.

library to the Academy. This library then held 591 volumes of journals and 531 books. Examination of the accession records shows that Morgan's library included almost every French or German book or journal concerned with physiology published between 1850 and 1865 and that this gift today comprises most of the holding in anatomy and physiology of the Academy for that period. Most of these books bear Morgan's signature and one of them was obviously sent to Morgan in New York by du Bois-Reymond; at any rate Morgan's name and address written in a hand resembling du Bois-Reymond's is carefully pasted inside the cover. No descendants of Morgan's parents survive today and although various collateral relatives have been very helpful, they have been unable to locate any family or personal documents on Morgan. A genealogy of the descendants of Thomas Morgan, Charles Morgan's grandfather, was published in 1917 in Norfolk by Reginald B. Henry, M. D. and is available in libraries.

The absence of information on Morgan's personal life is paralleled by an absence of information about his scientific life. There is no mention of him in the Ludwig-du Bois-Reymond correspondence, nor have I found any reference to his death in any German periodical. The somewhat derivative nature of Morgan's book raises the question of whether Morgan was interested in and capable of pursuing a research career as distinct from one of compilation. Several facts tend to answer this question affirmatively. Morgan did actually publish a research paper in Reichert's *Archiv* in 1863.[11] In this paper Morgan describes his studies on the question of whether or not the nerve sheath contributes significantly to the injury potential. Morgan says this problem suggested itself to him in the course of his preparing a version of du Bois-Reymond's book for the English speaking public. Morgan gave an expanded translation of this paper (which was printed in German and signed "Dr. Charles E. Morgan aus Amerika") on pages 463 ff. of his book. It is a competent piece of work on an important problem. It was referred to in a paper by du Bois-Reymond in the same volume of the *Archiv*,[12] and it was referred to at some length by Hermann in 1879.[13] In his book Morgan occa-

[11] Charles E. Morgan, "Einige Versuche mit dem Strom des ruhenden Nerven" *Arch. f. Anat. u. Physiol. und wissenschaftliche Medicin*, 1863: 338-344.

[12] Emil du Bois-Reymond "Ueber das Gesetz des Muskelstromes, mit besonderer Berücksichtigung des M. gastroknemius des Frosches" *Arch. f. Anat., Physiol. u. wissenschaftliche Medicin*: 584 n. (1863).

[13] Ludimar Hermann, *Handbuch der Physiologie*, Leipzig, Vogel, 1879. v. 2 part 1: "Allgemeine Nervenphysiologie," p. 169.

sionally referred to apparatus as being described from that in his own possession. The *Medical Record* says: "he had many projects on foot, and seemed to feel that his mission pointed toward original investigations in medicine, more especially to physiology. . . ." [14] Morgan did not publish any experimental work after his return to America, to the best of my knowledge, although he did publish a paper surveying modern thought in dermatology.[15] On the whole it seems as if Morgan would have followed a career in experimental physiology had he lived.

The obscurity into which this able and promising young investigator fell after his death obviously reflects the obscurity into which his chief claim to fame, his book, also fell. The contemporary reception of his book was favorable in a sense, but the reviews make the reasons for the neglect of his book quite clear. In general the reviews praised the book very highly as a work of great scientific stature quite beyond the grasp of the reviewer but a favorable omen for the future of science in America, deplored the style (which is often the result of over-literal translation), and deplored the absence of a more extensive treatment of therapeutics. The most thoroughly unfavorable review appeared in the *American Journal of the Medical Sciences*: [16]

> . . . his style is so obscure and involved, and the facts which he announces are presented in such a confused and unattractive manner, that while his readers will be few, those that read understandingly and with profit to themselves will be still fewer . . . the woodcuts, except in their occasional complicated obscurity, have not struck us as worthy of the text they are meant to illustrate. . . .

The preface to Morgan's book is by the brilliant neurologist, William A. Hammond, and in it he remarks: "Relative to the matter of the treatise, I may perhaps be permitted to say that there is nothing in the English language which at all approaches it as regards the scientific treatment of the whole subject of Electricity. It is a work which those who devote themselves to that study cannot do without, and is one which, for many years to come, must occupy the first place among the truly learned treatises on the important force, to the consideration of which it is devoted." Hammond was competent to judge, both as a former professional physiologist and as a student of electrotherapy. More than that, Hammond's career does not indicate that he was much given to saying things just to please

[14] *Op. cit.* (footnote 5).

[15] Charles E. Morgan, "Some Points in the Anatomy and Physiology of the Skin, and in the so-called Sympathies," *The Medical Record*, *2*:73-77 (1867).

[16] *The American Journal of the Medical Sciences*, N. S. *56*:246-7 (1868).

people. One of the journals edited by Hammond, *The New York Medical Journal*,[17] said: "we have for the first time a book on electricity which is complete, thorough, and exact as to its scientific side, with the most practical condensation of present knowledge as to electrical therapeutics. . . ." The *Medical Record*, although it praised Morgan's book, put the case for more therapeutics very strongly:[18]

> It contains nothing of the use of electricity in the relief of pain, nothing of its power as a constitutional tonic, nothing even of its employment in rheumatism or in the various types of inflammation. One might be able to recite the book by heart, accurately and understandingly, from the beginning to the end and yet be utterly at a loss when called upon to treat a case of St. Vitus' dance, of dyspepsia, of paralysis, or even of common facial neuralgia, by the electrical currents.

It would be hard, in the light of modern usage of electrotherapy, to find higher praise for Morgan's book, but such a review must have undermined sales to clinicians very drastically. An exactly opposite view appeared in *The Detroit Review of Medicine and Pharmacy*:[19] "The last twenty-five pages give the rules for the guidance of the practitioner, in the therapeutical application of electricity: and this brief space we deem the most significant comment upon our knowledge of this agent; of its power for good and evil we have no doubt, but we are yet in the infancy of our knowledge of its value." Still another journal under Hammond's editorship, the extremely interesting *Quarterly Journal of Psychological Medicine and Medical Jurisprudence*, was largely unenthusiastic in its review, although the reviewer did say that Morgan's book was[20] "One of the most eminently scientific works which this country has produced."

The most perceptive review appeared in *Lancet*.[21] Harsh but not malicious, it said in part:

> If it were not for the assurance of the editor, Dr. William A. Hammond, that the work had been completed by the author, we should have come to the conclusion that it contained the crude and diffuse notes of a singularly earnest student in the German schools; for it consists chiefly of an attempt to give in copious detail, although in very crippled English, the published results of German Electro-physiologists. . . . the work contains a mass of most valuable, although imperfectly

[17] *The New York Medical Journal*, *7*: 122-127 (1868).
[18] *The Medical Record*, *3*: 142-143 (1868).
[19] *The Detroit Review of Medicine and Pharmacy*, *3*: 392 (1868).
[20] *The Quarterly Journal of Psychological Medicine and Medical Jurisprudence*, *2*: 567-9 (1868).
[21] *Lancet*, 1868, vol. 2, p. 571.

digested matter. To the student who is not familiar with the German Language, the work will prove particularly useful. Mr. Morgan had mastered the theory, and largely the literature of his subject; and it is to be greatly regretted that his life was not spared to complete the exhaustive survey of it at which, it may be inferred, he aimed.

The idea that Morgan had not completed his work cannot be altogether rejected, although it seems that since he had already cut the blocks, at the most a single revision might have remained. Such a revision might of course have greatly improved the readability of the book.

There is only indirect evidence bearing on the sale of Morgan's book. The publishers carried it on their lists for many years and gave free copies to libraries in the 1890's. The book was expensive ($6.50 and 32/6), the reviews were not likely to stimulate sales, and in all probability sales were small. Williams and Wilkins, successors to William Wood, have no records on Morgan's book with the exception of an archive copy. The book was hardly ever cited in textbooks of physiology, e. g. no reference is made to it in Dalton, Foster, or Schaffer, and only one reference is made to it in Flint's five volumes. All in all its effect was apparently very small.

It is not hard to understand why such a work would have had very little success in the English speaking world of 1868. The appeal to clinicians was obviously minimal, and there were very few experimental physiologists at that time in England or the United States. Of those few, none were actively engaged in electrophysiology. The audience for the book simply was non-existent. The Germans on the other hand, familiar with their own literature, would not have turned to a survey of it in English. Indeed they might never have realized the nature of the work because of its misleading title. Had Morgan lived to send copies to Germany the story might have been different. As it was, I could find no review of the work in any German periodical.

Morgan's obscurity must be laid to his early death and the prematurity of his book. No book on physiology of similar scholarly stature was published in the United States in the 19th century, nor perhaps in England. No physiologist of similar training in physically oriented research appeared in the United States until some time after Morgan's death. One cannot help agreeing with the *New York Medical Journal* [22] that an examination of Morgan's book " will cause the reader to lament that its accomplished author has not lived to reap the honors which his great ability has

[22] *Op. cit.* (footnote 17).

won. It is one of a class of books very rare in this country, and one whose mere publication is a hopeful fact for that scientific future, which had he lived, Dr. Morgan would certainly have illustrated." [23]

[23] I have recently discovered that the John Crerar Library has many books which once belonged to du Bois-Reymond, including du Bois-Reymond's bound collection of his own reprints. In his reprint of the *Monatsberichte der könig. Akademie der Wissenschaften zu Berlin*, 18 July, 1867, du Bois-Reymond has written on the margin of p. 573 opposite the textual description of a piece of apparatus: "Morgan hat den App. abgebildet p. 372" Since on p. 372 of Morgan's book there is a figure of the apparatus in question, it is evident that du Bois-Reymond read Morgan's book. Unfortunately, the copy of Morgan's book in the Crerar Library is not du Bois-Reymond's copy. [Note added in proof.]

NATURE AND THE LIBIDO: A CASE OF SEMANTIC CONTINUITY

WILLIAM D. SHARPE

Webster's Unabridged Dictionary includes as a definition of ***nature***:

> The vital functions, processes, organs, or the like; also, one of these, esp. a generative or excretory organ, process, etc., or the matter issuing from such an organ;—now chiefly a euphemism; as, a call of *nature*; interference with *nature*.

This doesn't clearly refer to the *libido sexualis*. Similarity in expression does not by itself prove cultural influence, although philological descent is presumptive evidence in its favor, and in any event, the survival for more than twenty-five centuries of a word with one of its meanings unchanged would constitute a remarkable example of continuity amid a world of change. This seems to be the case in the Southern negro's use of *nature* when referring to the libido, often to the Northern medical neophyte's confusion as he attempts to unravel a satisfactory history from the totem and taboo of the genitourinary system. The word is used in such phrases as, "my nature been good since I was fourteen," or, "I lost my nature—might as well shoot me." The word is not ordinarily used in this sense by the white Southerner, save by an occasional elder of the lowest cultural class, though a Baltimore clergyman tells this writer that he was once mystified at being asked whether "loss of nature in my sleep" was sinful, until the enquirer's youthful voice suggested a reference to nocturnal emission. The clergyman could not recall whether the penitent was white or colored, or the accent definitely indigenous. Neither the late H. L. Mencken's scholarly *The American Language* nor any of the material consulted on American dialects mentions this—or any particular use of the word.

The Greek *physis*, Latin *natura* and English *nature* mean the same thing, much as *naus*, *navis* and *ship* stand for the same thing, and are not translations of each other. In Greek, *physis* stands for the libido often enough, as in Aristophanes' line, "Indulge your nature (*chrô têi physei*), drink and laugh: think nothing shameful of it,"[1] but can just as well refer to any instinctive behavior in animals or man, as in Democritus:

[1] Aristophanes, *Nubes*, 1078: the word nature here probably refers to libido.

For human beings it is one of the necessities of life to have children, arising from nature and primeval law (*apo physios kai katastasios tinos archaiês*). It is obvious in the other animals, too: they all have offspring by nature (*kata physin*), and not for the sake of any profit.[2]

The word has, of course, a much broader philosophical connotation—concerning which there have been many learned German articles—of nature as an originating power and the active principle of growth in the universe.[3] But the term *thêlus physa* is precisely rendered by " the female sex," and the word *physis* (translated as *natura* or *nature*) is applied to the sexual characteristics, commonly to the female genitalia though also to the male, as in the phrase *tôn duo physeôn* (in a scholion, as one would expect, to the *Lysistrata* of Aristophanes) referring to the two testes.

In early Latin, *physis* became *natura*, and instead of writing or compiling cosmological treatises *peri physeôs pantôn*, Latin writers from Lucretius to Isidore of Seville expounded *de natura rerum*—" On the Nature of Things." *Nature* still retained a host of subsidiary meanings, but this virtually equivalent descent of meanings is noteworthy. *Natura* can be defined as " the indecent parts of either sex, as *physis* among the Greeks," [4] and this definition is illustrated not only with reference to the Priapic Hymns,[5] but to that master of golden Latinity, M. Tullius Cicero himself: " [Mercury] is represented in a state of sexual excitement traditionally said to be due to passion inspired by the sight of Proserpina," [6] and " A certain married woman wanted to have a child, and, doubtful whether or not she was pregnant, dreamed that her womb (*natura*) had been sealed." [7] *Natura* and *physis* had the same meaning: the Latin language was remarkably poor in abstract nouns, a defect which impeded Roman speculative thought and which was supplied, really, only in post-Classical times, when *natura* acquired the connotation of an abstract sexual libido, apart from the concrete manifestations thereof.

[2] Democritus, Fragment 278, in Kathleen Freeman, *Ancilla to the Pre-Socratic Philosophers* (Oxford, 1952), p. 117.

[3] Cf. Cleanthes, *Stoica*, 1. 126.

[4] P. Pierrugues, *Glossarium eroticum linguae Latinae*. Second ed. (Berlin, 1908), p. 39.

[5] A particularly telling quotation, but one best retained in the decent obscurity of a learned tongue, is *Priapea carmina* 38, 1-2:

> Simpliciter tibi me, quodcumque est, dicere oportet:
> Natura est quoniam semper aperta mihi.

[6] Cicero, *De natura deorum*, 3. 22: Cujus [Mercurii] excitata natura traditur, quod adspectu Proserpinae commotus sit.

[7] Cicero, *De divinatione*, 2. 70: Parere quaedam matrona cupiens, dubitans essetne praegnans, visa est in quiete obsignatam habere naturam.

Later Latin shows an even clearer sexual connotation for *natura*: "that part of the body by which one is either a man or a woman" or, even more succinctly, "pro sexu" on such diverse authority as Cicero, the Laws of Henry I, King of England, and Vegetius' *Manual of Veterinary Surgery*.[8] *Naturalia* was the usual term for the female genitalia from classical Latin until very recently, and can still be found in some medical dictionaries.

This word is in none of the standard Anglo-Saxon and Middle English lexica, but seems to have been added to the English language by the medieval Latin clerks in response to a need for such an abstract noun. A Norman French intermediary can neither be postulated nor excluded on the basis of evidence available, but there seems to be no current use in French of such a term, and in any event, the caste distinctions imposed by the French-speaking conquerors upon the English-speaking vanquished relegated the venerably direct four-letter physiological monosyllables to an underground usage, thereby insuring their imperishability and early acquisition by every English-speaking child. The *Oxford English Dictionary*, under *nature*, refers to semen and the menses on the authority of writers from Chaucer down to the seventeenth century, a usage retained in Webster's dictionary. Chaucer's Poor Parson, preaching in 1386 about onanism, denounces it as an "unkyndely synne, by which man or woman shedeth her nature,"[9] and there is an earlier quotation which this writer has not been able to trace, from a sermon of about 1250, "*The* nature of Man is of greater streng*th*e and of greater hete in *tho* [= any] age."[10] The context is not exclusively sexual, but seems to include it.

In underworld slang, a mistress or prostitute, according to Partridge, was a *natural* from 1688 until 1800, but this usage had long ceased to be standard English by 1890, despite the survival of *natural child* for the more standard *bastard*.[11]

[8] Cf. *Glossarium manuale ad scriptores mediae et infimae Latinitatis* (Halle, 1772-1789), s. v. *Natura*.

[9] Chaucer, *The Parson's Tale*, 576.

[10] Though I was not able to locate the source of the *OED* citation, it is interesting that Robert Morris, *Old English Homilies of the Twelfth Century* (London, 1873), pp. 30-31, translates "On *th*e gole men he gel*th* wi*th* his herte. *and th*us que*th*. Gef *th*e is lef *th*in hele. heald *th*in cunde. *and th*ine licames lust kel ilome" as "In the heart of the lascivious man the devil sings, and thus saith:—If thou dost value thy health, follow nature, and cool frequently thy bodily lust."

[11] Eric Partridge. *A Dictionary of the Underworld: British and American* (New York, 1950), pp. 464-465, and *A Dictionary of Slang and Unconventional English*, Fourth ed. (New York, 1951), p. 553.

The word *nature* was often applied with various shades of meaning to the sexual parts, products and passions, surviving virtually unchanged from Early Greece until the twentieth century. It seems to have passed from late Latin into English in response to a need for an abstract noun to describe what is now termed the libido. There seems to be no English use of the word *nature* in an unequivocally sexual sense prior to Chaucer, who uses it as a common word, understood of the people: Chaucer's English, though, is closer to Shakespeare and Donne than to *Beowulf* and the *Anglo-Saxon Chronicle*. It is clear that *nature* referred to sexual parts and passions from Middle English until about 1800, retaining its former meaning somewhat longer. The American language felt the pinch of what passed for puritan morality more sharply and earlier than did the British language—to the point that bulls were once actually called " gentleman cows."

The first negro slaves in the Colonies learned English from their masters, and later, from their mothers: there were few fresh imports after the American Revolution, but until the abolition of that peculiar institution after the War Between the States, negroes were excluded from literary education. Their traditions and language were passed on orally, while Anglo-Saxon inscriptions in public places daily give witness to the survival of any tabooed nomenclature independent of literary media. Many considerations suggest that the negro *patois* is, in large measure, descended from seventeenth and eighteenth century colloquial English, much as the French of the Bayous or of Quebec hails from an earlier and more robust language than that of Voltaire and Rousseau. Oral usage changes but slowly without the influence of written examples: this usage of the word nature seems widespread through the South among negroes and the few whites of comparable social status: it is not apparently used for the libido among Louisiana negroes descended from French-speaking sires [12] and this would tend to corroborate the line of tradition presented above.

[12] Letters of Dr. R. H. Kampmeier, Nashville, to W. D. S., 20 June 1956; Dr. Edgar Hull, New Orleans, to W. D. S., 17 July 1956; and Dr. H. P. Chastant, Lafayette, Louisiana, to Dr. Edgar Hull, 24 July 1956.

MEDICO-HISTORICAL NEWS AND ACTIVITIES

ANNOUNCEMENTS

Benjamin Waterhouse Medical History Society

The Society will hold a meeting at the Massachusetts Memorial Hospital, April 23, 1957, at which Dr. Edward D. Churchill will speak on "Notes on the History of Boston Surgery."

College of Physicians of Philadelphia

On April 9, 1957, Mr. Robert Scott Stevenson, F. R. C. S., of Gibralter, United Kingdom, will deliver the annual Kate Hurd Mead Lecture, sponsored jointly by the Women's College of Pennsylvania and the College of Physicians. Mr. Stevenson will speak on "The Changing Scene in Harley Street."

Death of Capt. Pleadwell

Dr. Frank Lester Pleadwell, Captain (retired), U. S. Navy, died in Honolulu on January 30, 1957. Born August 9, 1872, in Taunton, Mass., he received his M. D. degree from Harvard in 1896. In the same year he became Assistant Surgeon, U. S. Navy, and followed a naval career until his retirement in 1929. In 1938 he was delegate of the U. S. Government and chairman of the American delegation at the International Congress of the History of Medicine. The author of various historical publications, Captain Pleadwell is known to the readers of this *Bulletin* through his articles on William Beaumont and Samuel Sorbière. Many members of the Association will remember him from past meetings of the Association.

Mayo Clinic

The Mayo Clinic Library, having been initiated in 1907, celebrates its fiftieth anniversary this year. *A Guide to the Use of the Mayo Clinic Library* has been prepared on this occasion.

Medical Library Association

The fifty-sixth annual meeting of the Association will take place at New York City, May 6-10, 1957. The program foresees a number of historical papers by George Rosen, Wilson G. Smillie, W. B. McDaniel II, and Estelle Brodman.

Meeting in Honor of the Memory of Dr. Sigerist

The Johns Hopkins Medical History Club, in association with the Institute of the History of Medicine, announces a special meeting on April 15, 1957, to honor the memory of Henry E. Sigerist. Speakers are: Ludwig Edelstein, Sanford Larkey, Richard Shryock, Owsei Temkin.

National Library of Medicine

The March 1957 issue of the National Library of Medicine *News* draws attention to its "Jubilee Preparations." The year 1961 will mark the 125th anniversary of the founding of the library; in 1962 fifty years will have passed since the death of Robert Fletcher, while 1963 represents both the 50th anniversary of the death of

John Shaw Billings and the 125th anniversary of his birth. A series of special publications is foreseen for the period of these years. In the probable sequence of appearance they are: a *Checklist of Medical Americana* by Mr. Robert B. Austin, and next, Dr. Dorothy M. Schullian's history of the National Library of Medicine; Dr. Estelle Brodman is preparing a memoir of Fletcher, and Lieutenant-Colonel Frank B. Rogers will make ready for publication the *Selected Papers of John Shaw Billings.*

New York

The State University of New York announces the third Symposium in its series dealing with the historical development of physiological thought for April 9-11, 1957. The Symposium, which is devoted to "The Broad Applications of Basic Concepts of Function in the Medical Sciences," comprises the following presentations:

April 9:

4:30 p. m.: "Development of the Concept of the Dynamic Process in Psychiatry," Gregory Zilboorg.

8:00 p. m.: "Modern Concepts on Biological Adaptation," Ludwig von Bertalanffy.

April 10:

4:30 p. m.: "What Keeps Men Alive? A Survey of the History of Thoughts about Life and Death," Bruno Kisch.

8:00 p. m.: "Towards the Philosophical Study of the Idea of Disease," Lelland J. Rather.

April 11:

4:30 p. m.: "The Contribution of Pharmacodynamics and Pharmacotherapy to Basic Physiological Thought," Alfred Gilman.

In commemoration of the 150th anniversary of the founding of the Medical Society of the State of New York in 1807, the February 1, 1957, number of the *New York State Journal of Medicine* appeared as a "Sesquicentennial Issue." It contains the program of the Sesquicentennial Convention of the Society as well as a medical chronicle of New York State comprising a great number of interesting historical articles.

Obituary Notes

Readers of the *Bulletin* may be interested in an article by Dr. Claudius F. Mayer on "Science history and its recent losses" that appeared in the February 1957 issue of *Military Medicine* and includes biographical sketches of medical historians recently deceased.

University of Chicago

Two talks on medical history have been arranged by the following four student organizations: Alpha Omega Alpha, Alpha Kappa Kappa, Nu Sigma Nu, Student A. M. A. The first lecture will be given on April 19, 1957, by Dr. E. M. K. Geiling on "William Henry Welch." On May 7, Dr. C. Howard Hatcher will speak on "Dallas B. Phemister."

BOOK REVIEWS

Blakiston's New Gould Medical Dictionary: A Modern Comprehensive Dictionary of the Terms Used in All Branches of Medicine and Allied Sciences. Edited by Normand L. Hoerr and Arthur Osol. Second edition. New York, Toronto, London: Blakiston Division, McGraw-Hill Book Company, Inc., 1956. xxvi + 1463 pp. Ill. $11.50.

One inevitably compares this edition with its immediate predecessor, now seven years old, which in turn was based on a succession of dictionaries going back many years which had enjoyed an enviable reputation. In the present volume much that was obsolete has been excluded to make room for the ever-expanding field of medical knowledge; despite this, almost 200 pages had to be added to an already large and heavy volume (the new dictionary weighs over six lbs.).

Like the first edition this edition is almost entirely a product of the Western Reserve University School of Medicine. The authors are to be congratulated upon producing such a fine example of medical lexicography. One is delighted with the typography, the clear lines, and the bold headings. The spacing is excellent and the use of a separate line for subdefinitions under a descriptive heading, e.g. syndrome, test, stain, etc., makes for ease in finding subsidiary terms, and is very welcome.

The decision to change to cloth covers is good as it may prolong the life of a much-used book, which this should become. Some may prefer the "leatherette" binding of its predecessor, though experience shows that the former will maintain a longer shelf-life without the back breaking as has happened to this reviewer's own dictionary.

Two features deserve special mention. The "figures," separately indexed in the middle of the volume, consist of 45 plates, more than half in color, which are beautiful examples of medical art, and excellent for quick reference. The appendix at the end of the volume covers well over 100 pages with tabulated data on anatomy, blood constituents, enzymes, hormones, isotopes, etc. These tables afford a ready means of review for the student and practitioner alike: indeed for the former they should prove invaluable. Interesting and unusual are the few pages defining the phobias. This is a fearsome society we are living in today!

The preface claims the inclusion of not only proprietaries and trade-name drugs but others still under investigation. The obvious impossibility of mentioning all such drugs leads one to question the advisability of including any. There is available annually to all doctors the *Physician's Desk Reference* which does list all these drugs, and also gives dosage and other important data, making reliance on the dictionary for this purpose entirely unnecessary. A few others not included are rauwiloid, resperoid, methium, pentoxylon, dactil, Visyneral (one of the first vitamin-mineral combinations), probanthine. One looks in vain for coricidin, for example, which is one of the earliest combinations of APC (which is defined) and an anti-histamine, chlor-trimeton, (also defined) for the treatment of the

early stages of a cold. A new concept in therapy surrounds the use of the citrus bio-flavanoids in maintaining capillary integrity, yet one does not find C. V. P. In fact the concept of capillary resistance is not mentioned, nor is the Göthlin index of capillary fragility included.

Some drugs from the ever-widening field of antibiotics are included, e. g. tetracyclines, oxytetracyclines; but no hint is given as to their areas of applicability. The jacket of the dictionary promises "new and exact definitions from many specialists in cardiology," etc., yet in the latter field the V leads 1 and 2 are defined as being derived by "placing electrodes in the fourth interspace" whereas the usual teaching is the second interspace. Hundreds of terms in various fields have been redefined and many new terms added, yet in psychiatry the obvious distinction between psychosomatic and somatopsychic is not made.

Definitions, in general, being accurate, it is surprising to find the meaning of vocal fremitus given as "the sounds of the voice transmitted *to the ear* when applied to the chest of a person speaking." Clearly this is an error, since vocal fremitus, indeed all fremitus, is a tactile perception, not an auditory one.

Of special interest to the readers of this periodical are the biographical data, by far the largest number of thumb-nail sketches available in any similar work. Here one encounters some disappointments in what appears to be unevenness or disparity in the subjects covered. Men of little or no historical "note" have been included simply because they are "remembered" for an operation, syndrome, test, even incision, while others whose names for one reason or another have not been attached to a symptom, syndrome, or muscle, are not even mentioned. While this dictionary is obviously not solely a biographical one nor a history of medicine, the fact that biographical material is included makes it necessary, it seems to this reviewer, to include quite a number of names which are conspicuous by their absence.

A few who would appear to "belong" here may be mentioned. Of a former generation there come to mind Moses Erdheim, the great Viennese pathologist, noted for his work on the parathyroids; Sir Morell Mackenzie, for his pioneer work in laryngology; Jacob Solis-Cohen, the first to perform successfully an operation for carcinoma of the larynx; Clarence E. McClung for his work in identifying the sex chromosome; Wiggers for his fundamental contributions in the physiology of the heart. The bufagins are defined but the discoverer of this toxic secretion of toads, David I. Macht, is not mentioned. Trudeau's pioneer contributions to the treatment of tuberculosis seem worthy of notice, as does Horatio C. Wood's discovery of the physiologic and therapeutic properties of hyoscine (1885), and introduction of atropin to combat the falling blood pressure in shock.

Among contemporary medical men omitted, one might mention Goldberger, for his study of unipolar leads; Wolferth and Wood, for their contributions to our knowledge of the chest leads in electrocardiography; Master, for his two-step exercise test to determine the presence of coronary insufficiency. Gofman, whose atherogenic index is mentioned, gets no notice, nor are the lipo-protein groups S_f 0-12 and S_f 12-400 defined. Few American surgeons have achieved such world-wide fame as has I. S. Ravdin, the only civilian physician to become a Brigadier-General of the Army, though, to be sure, there is no disease, incision or operation that bears his name.

The dictionary is naturally thumb-indexed but its use usually gets you the wrong letter, because two or three letters appear together; thus while JKL gives you J first, MN gives you N, OP gives you P, etc. In the former edition each letter was separately indexed, which seems better.

Usage has established the term atherosclerosis as meaning a particular type of hardening of the arteries. Despite this it is respectfully suggested that athero (porridge) and sclerosis (hardening) does not make sense (hardening of the athero). The term arteriosclerosis (sclerosis of the arteries) should be reserved for the particular pathologic condition usually implied by the definition. For the specific type with atheromatous degeneration the term might more appropriately be arterio-atherosis.

One final suggestion is made. It would seem advisable and certainly very helpful if the pronunciation of proper names were to be included, especially in view of the tremendous number of such items.

MAURICE S. JACOBS

E. B. SMITH. *Jean-Sylvain Bailly, Astronomer, Mystic, Revolutionary, 1736-1793.* Published as Volume 44, Part 4, of the *Transactions* of the American Philosophical Society, Philadelphia, 1954. 111 pp. Ill. $2.00.

In this study of Jean-Sylvain Bailly, Professor Edwin Burrows Smith of Wayne University has given us a useful work. While some accounts of Bailly's career already exist in English, such as Gene A. Brucker's *Jean-Sylvain Bailly, Revolutionary Mayor of Paris* (1950) and the translation of François Arago's *Biographies of Distinguished Scientific Men* (1857), in which some hundred pages are devoted to Bailly, the studies to date have either been too narrow in their scope or are simply long overdue for re-consideration. Professor Smith's book overcomes both these deficiencies, and its publication in the *Transactions* of the American Philosophical Society also makes it readily available to scholars everywhere.

Although never one of the first in any of the ranks in which the author enlists him, astronomer, mystic or revolutionary, Bailly served faithfully under all three banners, in a career that took him from a sheltered academic life to the mayoralty of Paris, to the turmoil of revolutionary politics, and to sudden death at the hands of the Terror. Perhaps more than with the really great, the men of original genius whose uniqueness sometimes separates them from their age, Bailly's life and work were wholly a reflection of his times. The winds of doctrine that blew through the Enlightenment sent the frail bark of his intellect scurrying in all directions. Scientist under the tutelage of Clairaut and La Caille, credulous disciple of Court de Gebelin, and "révolutionnaire sans le savoir," he seems to have always needed a guide and mentor.

Born in 1736 into a family ensconced in the Louvre for generations as *Gardes des Tableaux du Roi*, Bailly seemed destined for the same career. Indeed, he was named *Garde des Tableaux en survivance* in 1754 (not in 1768, as the author suggests, p. 430), and relieved of that rank in 1783 (not in 1785), at which time he was given a 1600 pound pension and the title of *Garde honoraire*. But Bailly was

hardly an artist, and almost nothing is known about his experience as an art critic, if any. When he was about twenty, Bailly met La Caille, and this first-hand contact with one of the most indefatigable astronomers of the eighteenth century fired him with an enthusiasm for astronomy. By 1760 he had published a paper on Halley's comet and had established an observatory of his own at the Louvre. Missing from the author's discussion of Bailly's work at this stage, is G. Bigourdan's useful article "Les instruments et le les observations de Bailly au Louvre," published in the *Comptes rendus de l'Académie des sciences* for 1920. Besides the opportunity to observe the predicted return of a comet early in his scientific career, Bailly also took part in the important transit of Venus observations of 1761, hopefully undertaken by the international scientific community of the eighteenth century to determine the basic astronomical unit, the solar parallax. These efforts, as well as others connected with lunar and stellar calculations, and studies of the satellites of Jupiter, brought him an appointment to the *Académie des sciences* as *adjoint-astronome* in 1763. His study of the satellites of Jupiter, which came to a climax with the publication of the *Essai sur la théorie des satellites de Jupiter* in 1776, is Bailly's most important scientific work. Thereafter his interests took another turn.

This is not the place to discuss those interests in detail, his history of ancient and modern astronomy, his letters on Plato's Atlantis, the essay on fables, or the style and content of his numerous literary portraits in the classic form which those biographical essays took during the Enlightenment, the *éloge*. As a student of French literature, Professor Smith discusses these sketches of men like Leibniz, Corneille, Molière, La Caille, and Captain Cook with analytical clarity and much skill. In the process, he highlights Bailly's own contribution to the eighteenth-century myth of the noble savage and the *sentiment de la nature*, that is contained in what is probably his poorest venture in biography, the *Éloge du capitaine Cook*. But the reader interested in the growing historiography of the history of science would also have welcomed a comparative analysis of say, Bailly's treatment of ancient astronomy with that of a nineteenth or twentieth-century student of the subject.

Throughout his career Bailly often found himself at home first with the sceptics and then with the credulous. Attracted by the one or the other, his writing alternately revealed this polar intellectual existence, resulting in the sceptical *Éloge de Leibniz* on the one hand, and the somewhat gullible *Histoire de l'astronomie ancienne* on the other. Fortunately, the rage for Franz Anton Mesmer and mental healing via animal magnetism that began in Paris with Mesmer's arrival there in 1778, found Bailly in one of his more doubting moods. The mysticism to which he was periodically prone—*frère illuminé* Condorcet had called him—was entirely absent when he was named to sit upon a commission (including Franklin and Lavoisier) composed of four doctors from the Paris Faculty and six members of the *Académie des sciences*, to investigate the claims of the mesmerists. The result was a denunciation of animal magnetism as a pure fraud imposed upon the innocent and the gullible. But the report of the commission, probably written by Bailly, went even further, and called attention to the profound influence of mind upon matter and the role of the imagination in individual or mass hysteria.

The work with this royal commission marked an important turning point in

Bailly's life. For a little while he continued to pursue an interest that mesmerism had precipited in his mind, the study of the imagination in man and animals—an effort that led to the letter " sur la question si les animaux sont entièrement privés d'imagination " of 1790. But serving ably with the investigating commission of 1784 brought him before the public eye and to the attention of the government. Within a year he was appointed to another commission for the reform of the Paris hospitals, and this was soon followed by membership on other government bodies concerned with the regulation of slaughterhouses and the arts and crafts, in a program that Professor Smith has aptly termed " reform by mandate." Between 1784 and 1789 he served his government broadly and efficiently, and the author is quite right when he says, for example, that the " reports on the hospitals . . . won him wide acclaim and probably account, more than any other single factor, for his election to the Estates General and his nomination as Mayor of Paris." (p. 507).

The capture of the revolution by the Terror eventually cost Bailly his life, as it did that of a far greater scientist, Lavoisier. A reform-minded courtier rather than a revolutionary, Bailly was swept aside by a tide of events whose power and depth and complexity he could not understand. To some extent, Professor Smith's assessment of his character after the report on mesmerism may also serve to evaluate the larger career of this unfortunate, first mayor of Paris. For in spite of many achievements already catalogued, Bailly " never managed to adopt the gentle scepticism of Franklin; he never achieved the purity of reflective thought of Diderot; he never learned to speak up like Condorcet and d'Alembert." (p. 493).

If Professor Smith's *Jean-Sylvain Bailly* is not the definitive biography of the astronomer, mystic, and revolutionary—and surely it cannot be where the author has been unable to consult any important manuscript sources—it is nevertheless as complete and well-done a book as those seeking general information about him are likely to demand. It is written with Cartesian clarity, and supplemented by a useful index and an excellent, comprehensive checklist of Bailly's printed works.

HARRY WOOLF

W. W. Francis: Tributes from his Friends on the Occasion of the Thirty-fifth Anniversary of the Osler Society of McGill University. Montreal: Published by the Society, 1956. Edition limited to 500 copies. xv + 123 pp. Ill.

In one of the most delightful of the thirty-seven tributes which go to make up this fascinating volume, Dr. E. P. Scarlett gives his reasons for departing from the usual pattern of the Festschrift essay. " If one followed custom," he writes, " it would be to turn out a treatise on some subject like the bleeding-bowls of the Middle Ages and present this with the dry tears of scholastic pedantry. But anything of this sort dedicated to W. W. F. could be nothing as stiff and sober as a treatise, but rather a gallimaufry, a glorious assembly of trivia, sacred and profane verse, sly vignettes, quips, nonsense, reflected flashes of wit and relics of reading—a transcript of life itself." Being at his wit's end to give anything like an adequate account of such a gloriously anomalous book as this, the reviewer unblushingly avails himself of this perfect description of its form and content.

The theme of nearly all the contributors is the part played by Dr. Francis in the preparation of the great catalogue of the Osler Library and (to quote again, from Miss Ruth Draper's charming tribute) his "faithful guardianship of the precious books and the precious associations that the Osler Library contains." The full story of the ten years' work on the catalogue is told by Reginald Hill, Francis' principal associate in the task, and by John Fulton, who witnessed, and took a not inconsiderable part in, the great undertaking during his Oxford days. The wider aspects of Dr. Francis's curatorship of the Osler Library, which proud office he has adorned since the arrival of the Library at McGill in 1929, are dealt with by the other contributors—physicians, surgeons, medical historians, and librarians. Every single essay contributes something to this picture of a great scholar, bibliographer, and entirely lovable "personality." Every essay bears the authentic stamp of a tribute from the heart. For all who have fallen under the Osler spell, this book about the Oslerator-in-Chief (as Dr. A. W. Franklin calls him) will be a sheer delight. Here we have the authentic stories of Francis reading to Osler in his bath, of his "imitations" of Dr. W. S. Thayer's French, of his puns and his songs, and of the "patter" with which he entertains visits to the Osler Library—and who can ever forget such a visit or, having once met him, forget the Keeper of the Shrine? Full justice is done to Dr. Francis's great work as an editor, bibliographer, and librarian. How many graduates and students of medicine owe to him their introduction to the delights of history and bibliography? How many of his friends in all parts of the world are indebted to what one contributor so aptly describes as the W. W. Francis Ready Reference Service. In 1924 Harvey Cushing wrote *à propos* of Dr. Francis's work on the incomparable Catalogue of the Bibliotheca Osleriana: "No other living person could do what Bill is doing, or dead person for that matter, not excepting W. O." Many of the contributors to this volume bear witness to the truth of that statement; and quite apart from the question of labour and scholarship there is the sheer genius of such notes as that on the date of death of Francesco Redi (*Bibl. Osleriana*, No. 5384 footnote).

In his address "On the Educational Value of the Medical Society" Osler quoted old Thomas Fuller on the bounden duty of a man to better his heritage. What the father found glass and made crystal, he urged the son to find crystal and make pearl. "Your heritage," said Osler to the members of the New Haven Medical Association, "has been most exceptional, and, I believe . . . that could your fathers return they would say that of this crystal you have made pearl." And if *he* could return would not Osler say the same of his kinsman? Every reader of the book will wish to associate himself in spirit with this tribute to a scholar who wears his learning so lightly, to the true "Transmitter" of the Osler tradition, and to a great librarian. No praise can be too high for the way in which Dr. Lloyd Stevenson and his co-editors, Drs. H. E. MacDermot and E. H. Bensley, have planned and carried through the production of the book. What a man!—And what a tribute! Those who are wise will hasten to secure one of the 500 copies before it is too late.

W. J. Bishop

ALFRED VOGT. *Theophrastus Paracelsus als Arzt und Philosoph.* Stuttgart: Hippocrates Verlag, 1956. 212 pp. D. M. 12.50.

This is a useful and well documented book. It is meant as an introduction to Paracelsus, in simple language, for the general reader and medical practitioner.

One of its commendable features is the generous space accorded to the general theosophical and anthropological speculations of Paracelsus, as embodied in the *Astronomia Magna* (" Philosophia Sagax of the greater and lesser World "). The main results of the lucid interpretation can be summarised as follows: Man is influenced by the stars. But these do not act as individual beings; it is the Cosmos as a whole that influences and indeed conditions man. In this lies the original trait in which Paracelsus' speculation differs from the astrological bias of Renaissance anthropology. A further point is Paracelsus' interpretation of man's participation in the Cosmos in naturalistic terms. The spirit of divine wisdom is present everywhere in the world as a " substance," not as a remote Platonic " ideal " pattern or counterpart. Consequently, not only belief but also the study of nature leads the way to God. In the religious field Paracelsus aimed at a belief that ensures contact with God more intimate than through the mediation of Christ. He speaks more of God than of Christ; he confesses to a " heathen " character of his science and medicine—but nature remains the realm in which God's miraculous deeds are manifested. " Nature " and believing " Soul " are thus united in God, indeed the powers of the soul are like the virtues of nature. Paracelsus thus prepared a place for proto-scientific naturalism in the framework of religious and " mythical " thinking. This, in the author's opinion, led to an extrication of the material world from religion and philosophy, already in Paracelsus' own work—it amounts to an emancipation of the scientific (chemical) investigation even from his own " astrosophy " and cosmology. As the author sees it, Paracelsus thus replaced alchemy by chemistry, and based pathology on the physico-chemical study of combustion. " To have recognised what is absolutely dead—the ultima materia—is the merit that Paracelsus can claim in the history of medicine and science . . . since Paracelsus the history of chemistry is the history of *Ent-Organisierung* (restricting and abolishing the prerogative of the organic), the history of the search for death in the living " (p. 100).

If acceptable, this would be an easy solution of the Paracelsus problem. However, Paracelsus pursued with equal vigour the search for life in the " dead " inorganic world—as his vitalistic concept of the Archeus shows. Moreover, to establish any easy formula for Paracelsus as a phenomenon in history implies resignation of the task confronting the historian: to integrate the religious and speculative element with his proto-scientific trends and achievements, even in chemical, physiological, and pathological detail. The author emphasises a dualistic " break " in the world of Paracelsus—between the realm of the religious-spiritual and that of Nature with her own and independent laws (p. 127). By contrast, in the reviewer's opinion, a monistic tendency is even more noticeable in Paracelsus—a tendency to see the spiritual in the corporeal and vice versa.

However, this in no way detracts from the distinct merits of the book as a simple introduction to Paracelsus and a clear presentation of his progressive views—notably in chemical and surgical pathology and therapy.

W. PAGEL

THE BULLETIN RECENTLY RECEIVED *

Torbern Bergman. *Om luftsyra* (Utdrag ur KVA 1773); *Afhandling om Bitter- Selzer- Spa- och Pyrmonter- Vatten, samt deras tilredande genom konst* (Utdrag ur KVA 1775) [both printed in facsimile].—Uno Boklund. *Torbern Bergman som pioniär på mineralvattenområdet.* Stockholm: Almqvist & Wiksell, 1956. 127 pp. Ill.

Torbern Bergman. *On Acid of Air* (Excerpt from *KVA* 1773); *Treatise on Bitter, Seltzer, Spa and Pyrmont Waters and Their Synthetical Preparation* (Excerpt from KVA 1775).—Uno Boklund. *Torbern Bergman as Pioneer in the Domain of Mineral Waters.* Translated by Sven M. Jonsson. Stockholm: Almqvist & Wiksell, 1956. 131 pp. Ill.

Thomas Neville Bonner. *Medicine in Chicago, 1850-1950.* A Chapter in the Social and Scientific Development of a City. Madison, Wis.: The American History Research Center, 1957. xvii + 302 pp. Ill. $5.00.

Samuel James Crowe. *Halsted of Johns Hopkins.* The Man and His Men. Springfield, Ill.: Charles C. Thomas, 1957. ix + 247 pp. Ill. $5.00.

George Haines, IV. *German Influence upon English Education and Science, 1800-1866.* New London, Conn.: Connecticut College, 1957. xii + 106 pp. $3.50.

The Jamnagar Experiment. The Central Institute of Research in Indigenous Systems of Medicine, Jamnagar, Saurashta, India, 1956. 36 pp. Ill.

Alexandre Koyré. *From the Closed World to the Infinite Universe.* Baltimore: The Johns Hopkins Press, 1957. xii + 313 pp. Ill. $5.00.

Donald R. McNeil. *The Fight for Fluoridation.* New York: Oxford University Press, 1957. xii + 241 pp. $5.00.

P. M. Mehta. The History of Indian Medicine. Reprint from *The Antiseptic,* October, 1956, *53*: 725-732.

Hugo Olsson. Torbern Bergman, 1935-1784. Reprinted from *Torbern Bergman's Foreign Correspondence.* Uppsala: Almqvist & Wiksells, 1956. 8 pp.

Vasco Ronchi. *Optics, the Science of Vision.* Translated from the Italian and revised by Edward Rosen. New York: New York University Press, 1957. ix + 360 pp. Ill. $10.00.

George Sarton. *Six Wings.* Men of Science in the Renaissance. Bloomington: Indiana University Press, 1957. xiv + 318 pp. Ill. $6.75.

Hermann Triepel. *Die anatomischen Namen.* Ihre Ableitung und Aussprache. 25. Auflage, völlig neu bearbeitet . . . von . . . Robert Herrlinger. München: Verlag von J. F. Bergmann, 1957. 82 pp. D.M. 6.80.

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* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

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